HISTORY

OF

SCIENTIFIC IDEAS.

VOLUME II.

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HISTORY

OF

SCIENTIFIC IDEAS.

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BEING THE FIRST PART OF THE PHILOSOPHY
OF THE INDUCTIVE SCIENCES.

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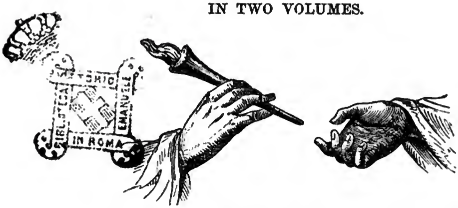
IN TWO VOLUMES.



ΛΑΜΠΑΔΙΑ ΕΧΟΝΤΕΣ ΔΙΑΔΩΣΟΥΣΙΝ ΑΛΛΗΛΟΙΣ.

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A PHILOSOPHER was asked:—How much does smoke weigh? He answered: Subtract from the weight of the fuel the weight of the ashes, and thou hast the weight of the smoke. Thus he assumed as incontrovertible that, even in the fire, the Substance does not perish, only its Form undergoes a change. In like manner the proposition, *Nothing can come of Nothing*, was only another consequence of the Principle of Permanence, or rather of the Principle of the Enduring Existence of the same subject with different appearances.

KANT, *Kritik d. r. Vern.*

BOOK VI.

THE PHILOSOPHY OF CHEMISTRY.

CHAPTER I.

Attempts to conceive Elementary Composition.

1. WE have now to bring into view, if possible, the Ideas and General Principles which are involved in Chemistry,—the science of the composition of bodies. For in this as in other parts of human knowledge, we shall find that there are certain Ideas, deeply seated in the mind, though shaped and unfolded by external observation, which are necessary conditions of the existence of such a science. These Ideas it is, which impel man to such a knowledge of the Composition of bodies, which give *meaning* to *facts* exhibiting this composition, and *universality* to *special* truths discovered by experience. These are the Ideas of *Element* and of *Substance.*

Unlike the Idea of Polarity, of which we treated in the last Book, these Ideas have been current in men's minds from very early times, and formed the subject of some of the first speculations of philosophers. It happened however, as might have been expected, that in the first attempts they were not clearly distinguished from other notions, and were apprehended and applied in an obscure and confused manner. We cannot better exhibit the peculiar character and meaning of these Ideas than by tracing the form which they have assumed

and the efficacy which they have exerted in these successive essays. This, therefore, I shall endeavour to do, beginning with the Idea of Element.

2. That bodies are composed or made up of certain parts, elements, or principles, is a conception which has existed in men's minds from the beginning of the first attempts at speculative knowledge. The doctrine of the Four Elements, Earth, Air, Fire and Water, of which all things in the universe were supposed to be constituted, is one of the earliest forms in which this conception was systematized; and this doctrine is stated by various authors to have existed as early as the times of the ancient Egyptians[1]. The words usually employed by Greek writers to express these elements are ἀρχὴ a *principle* or *beginning*, and στοιχεῖον, which probably meant a *letter* (of a word) before it meant an *element* of a compound. For the resolution of a word into its letters is undoubtedly a remarkable instance of a successful analysis performed at an early stage of man's history; and might very naturally supply a metaphor to denote the analysis of substances into their intimate parts, when men began to contemplate such an analysis as a subject of speculation. The Latin word *elementum* itself, though by its form it appears to be a derivative abstract term, comes from some root now obsolete; probably[2] from a word signifying *to grow* or *spring up*.

The mode in which elements form the compound bodies and determine their properties was at first, as might be expected, vaguely and variously conceived. It will, I trust, hereafter be made clear to the reader that the relation of the elements to the compound involves a peculiar and appropriate Fundamental Idea, not susceptible of being correctly represented by any comparison or combination of other ideas, and guiding us to clear and definite results only when it is illustrated

[1] Gilbert's *Phys.* l. i. c. iii.

[2] Vossius *in voce.* "Conjecto esse ab antiqua voco *eleo* pro *oleo*, id est *cresco:* a qua significatione *proles, suboles, adolescens:* ut ab *juratum, juramentum;* ab *adjutum, adjumentum:* sic ab *eletum, elementum:* quia inde omnia crescunt ac nascuntur."

and nourished by an abundant supply of experimental facts. But at first the peculiar and special notion which is required in a just conception of the constitution of bodies was neither discerned nor suspected; and up to a very late period in the history of chemistry, men went on attempting to apprehend the constitution of bodies more clearly by substituting for this obscure and recondite idea of Elementary Composition, some other idea more obvious, more luminous, and more familiar, such as the ideas of Resemblance, Position, and mechanical Force. We shall briefly speak of some of these attempts, and of the errours which were thus introduced into speculations on the relations of elements and compounds.

3. *Compounds assumed to resemble their Elements.*—The first notion was that compounds derive their qualities from their elements by *resemblance*:—they are hot in virtue of a hot element, heavy in virtue of a heavy element, and so on. In this way the doctrine of the *four elements* was framed; for every body is either hot or cold, moist or dry; and by combining these qualities in all possible ways, men devised four elementary substances, as has been stated in the History[3].

This assumption of the derivation of the qualities of bodies from similar qualities in the elements was, as we shall see, altogether baseless and unphilosophical, yet it prevailed long and universally. It was the foundation of medicine for a long period, both in Europe and Asia; disorders being divided into hot, cold, and the like; and remedies being arranged according to similar distinctions. Many readers will recollect, perhaps, the story[4] of the indignation which the Persian physicians felt towards the European, when he undertook to cure the ill effects of cucumber upon the patient, by means of mercurial medicine: for cucumber, which is cold, could not be counteracted, they maintained, by mercury, which in their classification is cold also. Similar views of the operation of medicines might

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[3] *Hist. Ind. Sc.* b. i. c. ii. sec. 2.

[4] See *Hadji Baba.*

easily be traced in our own country. A moment's reflection may convince us that when drugs of any kind are subjected to the chemistry of the human stomach and thus made to operate on the human frame, it is utterly impossible to form the most remote conjecture what the result will be, from any such vague notions of their qualities as the common use of our senses can give. And in like manner the common operations of chemistry give rise, in almost every instance, to products which bear no resemblance to the materials employed. The results of the furnace, the alembic, the mixture, frequently have no visible likeness to the ingredients operated upon. Iron becomes steel by the addition of a little charcoal; but what visible trace of the charcoal is presented by the metal thus modified? The most beautiful colours are given to glass and earthenware by minute portions of the ores of black or dingy metals, as iron and manganese. The worker in metal, the painter, the dyer, the vintner, the brewer, all the artisans in short who deal with practical chemistry, are able to teach the speculative chemist that it is an utter mistake to expect that the qualities of the elements shall be still discoverable, in an unaltered form, in the compound. This first rude notion of an element, that it determines the properties of bodies *by resemblance*, must be utterly rejected and abandoned before we can make any advance towards a true apprehension of the constitution of bodies.

4. This step accordingly was made, when the hypothesis of the four elements was given up, and the doctrine of the *three Principles*, Salt, Sulphur, and Mercury, was substituted in its place. For in making this change, as I have remarked in the History[5], the real advance was the acknowledgment of the changes, produced by the chemist's operations, as results to be accounted for by the union and separation of substantial elements, however great the changes, and however unlike the product might be to the materials. And this step once made, chemists went on constantly

[5] *Hist. Ind. Sc.* b. iv. c. 1.

advancing towards a truer view of the nature of an element, and consequently, towards a more satisfactory theory of chemical operations.

5. Yet we may, I think, note one instance, even in the works of eminent modern chemists, in which this maxim, that we have no right to expect any resemblance between the elements and the compound, is lost sight of. I speak of certain classifications of mineral substances. Berzelius, in his System of Mineral Arrangement, places *sulphur* next to the *sulphurets.* But surely this is an errour, involving the ancient assumption of the resemblance of elements and compounds; as if we were to expect the sulphurets to bear a resemblance to sulphur. All classifications are intended to bring together things resembling each other: the sulphurets of metals have certain general resemblances to each other which make them a tolerably distinct, well determined, class of bodies. But sulphur has no resemblances with these, and no analogies with them, either in physical or even in chemical properties. It is a simple body; and both its resemblances and its analogies direct us to place it along with other simple bodies, (selenium, and phosphorus,) which, united with metals, produce compounds not very different from the sulphurets. Sulphur cannot be, nor approach to being, a sulphuret; we must not confound what it *is* with what it *makes.* Sulphur has its proper influence in determining the properties of the compound into which it enters; but it does not do this according to resemblance of qualities, or according to any principle which properly leads to propinquity in classification.

6. *Compounds assumed to be determined by the Figure of Elements.*—I pass over the fanciful modes of representing chemical changes which were employed by the Alchemists; for these strange inventions did little in leading men towards a juster view of the relations of elements to compounds. I proceed for an instant to the attempt to substitute another obvious conception for the still obscure notion of elementary composition. It was imagined that all the properties of bodies and their mutual operations might be

accounted for by supposing them constituted of *particles* of various *forms*, round or angular, pointed or hooked, straight or spiral. This is a very ancient hypothesis, and a favourite one with many casual speculators in all ages. Thus Lucretius undertakes to explain why wine passes rapidly through a sieve and oil slowly, by telling us that the latter substance has its particles either larger than those of the other, or more hooked and interwoven together. And he accounts for the difference of sweet and bitter by supposing the particles in the former case to be round and smooth, in the latter sharp and jagged[6]. Similar assumptions prevailed in modern times on the revival of the mechanical philosophy, and constitute a large part of the physical schemes of Descartes and Gassendi. They were also adopted to a considerable extent by the chemists. Acids were without hesitation assumed to consist of sharp pointed particles; which, 'I hope,' Lemery says[7], 'no one will dispute, seeing every one's experience does demonstrate it: he needs but taste an acid to be satisfied of it, for it pricks the tongue like anything keen and finely cut.' Such an assumption is not only altogether gratuitous and useless, but appears to be founded in some degree upon a confusion in the metaphorical and literal use of such words as *keen* and *sharp*. The assumption once made, it was easy to accommodate it, in a manner equally arbitrary, to other facts. 'A demonstrative and convincing proof that an acid does consist of pointed parts is, that not only all acid salts do crystallize into edges, but all dissolutions of different things, caused by acid liquors, do assume this figure in their crystallization. These crystals consist of points differing both in length and bigness one from another, and this diversity must be attributed to the keener or blunter edges of the different sorts of acids: and so likewise this difference of the points in subtilty is the cause that one acid can penetrate and dissolve with one sort of *mixt*, that another can't rarify at all: Thus *vinegar* dissolves *lead*,

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[6] *De Rerum Natura*, ii. 390 sqq.   [7] *Chemistry*, p. 25.

which *aqua fortis* can't: *aqua fortis* dissolves *quicksilver*, which *vinegar* will not touch; *aqua regalis* dissolves *gold*, whenas *aqua fortis* cannot meddle with it; on the contrary, *aqua fortis* dissolves *silver*, but can do nothing with gold, and so of the rest.'

The leading fact of the vehement combination and complete union of acid and alkali readily suggested a fit form for the particles of the latter class of substances. 'This effect,' Lemery adds, 'may make us reasonably conjecture that an alkali is a terrestrious and solid matter whose forms are figured after such a manner that the acid points entering in do strike and divide whatever opposes their motion.' And in a like spirit are the speculations in Dr. Mead's *Mechanical Account of Poisons* (1745). Thus he explains the poisonous effect of *corrosive sublimate* of mercury by saying[8] that the particles of the salt are a kind of lamellæ or blades to which the mercury gives an additional weight. If resublimed with three-fourths the quantity of mercury, it loses its corrosiveness, (becoming *calomel*,) which arises from this, that in sublimation 'the crystalline blades are divided every time more and more by the force of the fire:' and 'the broken pieces of the crystals uniting into little masses of differing figures from their former make, those cutting points are now so much smaller that they cannot make wounds deep enough to be equally mischievous and deadly: and therefore do only vellicate and twitch the sensible membranes of the stomach.'

7. Among all this very fanciful and gratuitous assumption we may notice one true principle clearly introduced, namely, that the suppositions which we make respecting the forms of the elementary particles of bodies and their mode of combination must be such as to explain the facts of crystallization, as well as of mere chemical change. This principle we shall hereafter have occasion to insist upon further.

I now proceed to consider a more refined form of assumption respecting the constitution of bodies, yet

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[8] P. 199.

still one in which a vain attempt is made to substitute for the peculiar idea of chemical composition a more familiar mechanical conception.

8. *Compounds assumed to be determined by the Mechanical Attraction of the Elements.*—When, in consequence of the investigations and discoveries of Newton and his predecessors, the conception of mechanical force had become clear and familiar, so far as the action of external forces upon a body was concerned, it was very natural that the mathematicians who had pursued this train of speculation should attempt to apply the same conception to that mutual action of the internal parts of a body by which they are held together. Newton himself had pointed the way to this attempt. In the Preface to the *Principia*, after speaking of what he has done in calculating the effects of forces upon the planets, satellites, &c., he adds, 'Would it were permitted us to deduce the other phenomena of nature from mechanical principles by the same kind of reasoning. For many things move me to suspect that all these phenomena depend upon certain forces, by which the particles of bodies, through causes not yet known, are either urged towards each other, and cohere according to regular figures, or are repelled and recede from each other; which forces being unknown, philosophers have hitherto made their attempts upon nature in vain.' The same thought is at a later period followed out further in one of the Queries at the end of the Opticks[9]. 'Have not the small particles of bodies certain Powers, Virtues, or Forces, by which they act at a distance, not only upon the rays of light for reflecting, refracting and inflecting them, but also upon one another for producing a great part of the phenomena of nature?' And a little further on he proceeds to apply this expressly to chemical changes. 'When Salt of Tartar runs *per deliquium* [or as we now express it, deliquesces] is not this done by an *attraction* between the particles of the Salt of Tartar and the particles of the water which float in the air in

[9] Query 31.

the form of vapours? And why does not common salt, or saltpetre, or vitriol, run *per deliquium*, but for want of such an attraction? or why does not Salt of Tartar draw more water out of the air than in a certain proportion to its quantity, but for want of an attractive force after it is saturated with water?' He goes on to put a great number of similar cases, all tending to the same point, that chemical combinations cannot be conceived in any other way than as an attraction of particles.

9. Succeeding speculators in his school attempted to follow out this view. Dr. Frend, of Christ Church, in 1710, published his *Prælectiones Chymicæ, in quibus omnes fere Operationes Chymicæ ad vera Principia ex ipsius Naturæ Legibus rediguntur. Oxonii habitæ.* This book is dedicated to Newton, and in the dedication, the promise of advantage to chemistry from the influence of the Newtonian discoveries is spoken of somewhat largely,—much more largely, indeed, than has yet been justified by the sequel. After declaring in strong terms that the only prospect of improving science consists in following the footsteps of Newton, the author adds, 'That force of attraction, of which you first so successfully traced the influence in the heavenly bodies, operates in the most minute corpuscles, as you long ago hinted in your *Principia*, and have lately plainly shown in your *Opticks;* and this force we are only just beginning to perceive and to study. Under these circumstances I have been desirous of trying what is the result of this view in chemistry.' The work opens formally enough, with a statement of general mechanical principles, of which the most peculiar are these:—'That there exists an attractive force by which particles when at very small distances from each other, are drawn together;—that this force is different, according to the different figure and density of the particles;—that the force may be greater on one side of a particle than on the other;—that the force by which particles cohere together arises from attraction, and is variously modified according to the quantity of contacts.' But these principles are not

applied in any definite manner to the explanation of specific phenomena. He attempts, indeed, the question of special solvents[10]. Why does *aqua fortis* dissolve silver and not gold, while *aqua regia* dissolves gold and not silver? which, he says, is the most difficult question in chemistry, and which is certainly a fundamental question in the formation of chemical theory. He solves it by certain assumptions respecting the forces of attraction of the particles, and also the diameter of the particles of the acids and the pores of the metals, all which suppositions are gratuitous.

10. We may observe further, that by speaking, as I have stated that he does, of the figure of particles, he mixes together the assumption of the last section with the one which we are considering in this. This combination is very unphilosophical, or, to say the least, very insufficient, since it makes a new hypothesis necessary. If a body be composed of cubical particles, held together by their mutual attraction, by what force are the parts of each cube held together? In order to understand their structure, we are obliged again to assume a cohesive force of the second order, binding together the particles of each particle. And therefore Newton himself says[11], very justly, 'The parts of all homogeneal hard bodies which fully touch each other, stick together very strongly: and for explaining how this is, some have invented hooked atoms, *which is begging the question.*' For (he means to imply,) how do the parts of the hook stick together?

The same remark is applicable to all hypotheses in which particles of a complex structure are assumed as the constituents of bodies: for while we suppose bodies and their known properties to result from the mutual actions of these particles, we are compelled to suppose the parts of each particle to be held together by forces still more difficult to conceive, since they are disclosed only by the properties of these particles, which as yet are unknown. Yet Newton himself has not abstained from such hypotheses: thus he says[12], 'A particle of

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[10] P. 54. [11] *Opticks*, p. 364. [12] *Opticks*, p. 362.

a salt may be compared to a chaos, being dense, hard, dry, and earthy in the center, and moist and watery in the circumference.'

Since Newton's time the use of the term *attraction*, as expressing the cause of the union of the chemical elements of bodies, has been familiarly continued; and has, no doubt, been accompanied in the minds of many persons with an obscure notion that chemical attraction is, in some way, a kind of mechanical attraction of the particles of bodies. Yet the doctrine that *chemical* 'attraction' and *mechanical* attraction are forces of the same kind has never, so far as I am aware, been worked out into a system of chemical theory; nor even applied with any distinctness as an explanation of any particular chemical phenomena. Any such attempt, indeed, could only tend to bring more clearly into view the entire inadequacy of such a mode of explanation. For the leading phenomena of chemistry are all of such a nature that no mechanical combination can serve to express them, without an immense accumulation of additional hypotheses. If we take as our problem the changes of colour, transparency, texture, taste, odour, produced by small changes in the ingredients, how can we expect to give a mechanical account of these, till we can give a mechanical account of colour, transparency, texture, taste, odour, themselves? And if our mechanical hypothesis of the elementary constitution of bodies does not explain *such* phenomena as those changes, what can it explain, or what can be the value of it? I do not here insist upon a remark which will afterwards come before us, that even crystalline form, a phenomenon of a far more obviously mechanical nature than those just alluded to, has never yet been in any degree explained by such assumptions as this, that bodies consist of elementary particles exerting forces of the same nature as the central forces which we contemplate in Mechanics.

When therefore Newton asks, 'When some stones, as spar of lead, dissolved in proper menstruums, become salts, do not these things show that salts are dry earth and watery acid united by *attraction?*' we may

answer, that this mode of expression appears to be intended to identify chemical combination with mechanical attraction;—that there would be no objection to any such identification, if we could, in that way, explain, or even classify well, a collection of chemical facts; but that this has never yet been done by the help of such expressions. Till some advance of this kind can be pointed out, we must necessarily consider the power which produces chemical combination as a peculiar principle, a special relation of the elements, not rightly expressed in mechanical terms. And we now proceed to consider this relation under the name by which it is most familiarly known.

CHAPTER II.

Establishment and Development of the Idea of Chemical Affinity.

1. THE earlier chemists did not commonly involve themselves in the confusion into which the mechanical philosophers ran, of comparing chemical to mechanical forces. Their attention was engaged, and their ideas were moulded, by their own pursuits. They saw that the connexion of elements and compounds with which they had to deal, was a peculiar relation which must be studied directly; and which must be understood, if understood at all, in itself, and not by comparison with a different class of relations. At different periods of the progress of chemistry, the conception of this relation, still vague and obscure, was expressed in various manners; and at last this conception was clothed in tolerably consistent phraseology, and the principles which it involved were, by the united force of thought and experiment, brought into view.

2. The power by which the elements of bodies combine chemically, being, as we have seen, a peculiar agency, different from mere mechanical connexion or attraction, it is desirable to have it designated by a distinct and peculiar name; and the term *Affinity* has been employed for that purpose by most modern chemists. The word 'affinity' in common language means, sometimes resemblance, and sometimes relationship and ties of family. It is from the latter sense that the metaphor is borrowed when we speak of 'chemical affinity.' By the employment of this term we do not indicate a resemblance, but a disposition to unite. Using the word in a common unscientific manner, we might say that chlorine, bromine, and iodine, have a great

natural affinity with each other, for there are considerable resemblances and analogies among them; but these bodies have very little *chemical* Affinity for each other. The use of the word in the *former* sense, of resemblance, can be traced in earlier chemists; but the word does not appear to have acquired its peculiar *chemical* meaning till after Boerhaave's time. Boerhaave, however, is the writer in whom we first find a due apprehension of the peculiarity and importance of the Idea which it now expresses. When we make a chemical solution[1], he says, not only are the particles of the dissolved body separated from each other, but they are closely united to the particles of the solvent. When *aqua regia* dissolves gold, do you not see, he says to his hearers, that there must be between each particle of the solvent and of the metal, a mutual virtue by which each loves, unites with, and holds the other (*amat, unit, retinet*)? The opinion previously prevalent had been that the solvent merely separates the parts of the body dissolved: and most philosophers had conceived this separation as performed by mechanical operations of the particles, resembling, for instance, the operation of wedges breaking up a block of timber. But Boerhaave forcibly and earnestly points out the insufficiency of the conception. This, he says, does not account for what we see. We have not only a separation, but a new combination. There is a force by which the particles of the solvent associate to themselves the parts dissolved, not a force by which they repel and dissever them. We are here to imagine not mechanical action, not violent impulse, not antipathy, but love, at least if love be the desire of uniting. (Non igitur hic etiam actiones mechanicæ, non propulsiones violentæ, non inimicitiæ cogitandæ, sed amicitiæ, si amor dicendus copulæ cupido.) The novelty of this view is evidenced by the mode in which he apologizes for introducing it. 'Fateor, paradoxa hæc assertio.' To Boerhaave, therefore, (especially considering his great influence as a teacher of chemistry,) we may

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1 *Elementa Chemiæ*. Lugd. Bat. 1732, p. 677.

assign the merit of first diffusing a proper view of Chemical Affinity as a peculiar force, the origin of almost all chemical changes and operations.

3. To Boerhaave is usually assigned also the credit of introducing the *word* 'Affinity' among chemists; but I do not find that the word is often used by him in this sense; perhaps not at all[2]. But however this may be, the term is, on many accounts, well worthy to be preserved, as I shall endeavour to show. Other terms were used in the same sense during the early part of the eighteenth century. Thus when Geoffroy, in 1718, laid before the Academy of Paris his Tables of Affinities, which perhaps did more than any other event to fix the Idea of Affinity, he termed them 'Tables of the Relations of Bodies;' '*Tables des Rapports:*' speaking however, also, of their 'disposition to unite,' and using other phrases of the same import.

The term *attraction*, having been recommended by Newton as a fit word to designate the force which produces chemical combination, continued in great favour in England, where the Newtonian philosophy was looked upon as applicable to every branch of science. In France, on the contrary, where Descartes still reigned triumphant, 'attraction,' the watch-word of the enemy, was a sound never uttered but with dislike and suspicion. In 1718 (in the notice of Geoffroy's Table,) the Secretary of the Academy, after pointing out some of the peculiar circumstances of chemical

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[2] See Dumas, *Leçons de Phil. Chim.* p. 364. Rees' *Cyclopædia*, Art. Chemistry. In the passage of Boerhaave to which I refer above, *affinitas* is rather opposed to, than identified with, chemical combination. When, he says, the parts of the body to be dissolved are dissevered by the solvent, why do they remain united to the particles of the solvent, and why do not rather both the particles of the solvent and of the dissolved body collect into homogeneous bodies by their *affinity?* 'denuo se affinitate suæ naturæ colligant in corpora homogenea?' And the answer is, because they possess another force which counteracts this affinity of homogeneous particles, and makes compounds of different elements. Affinity, in chemistry, now means the tendency of *different* kinds of matter to unite: but it appears, as I have said, to have acquired this sense since Boerhaave's time.

combinations, says, 'Sympathies and attractions would suit well here, if there were such things.' 'Les sympathies, les attractions conviendroient bien ici, si elles étaient quelque chose.' And at a later period, in 1731, having to write the *éloge* of Geoffroy after his death, he says, 'He gave, in 1718, a singular system, and a Table of *Affinities*, or Relations of the different substances in chemistry. These affinities gave uneasiness to some persons, who feared that they were *attractions in disguise*, and all the more dangerous in consequence of the seductive forms which clever people have contrived to give them. It was found in the sequel that this scruple might be got over.'

This is the earliest published instance, so far as I am aware, in which the word 'Affinity' is distinctly used for the cause of chemical composition; and taking into account the circumstances, the word appears to have been adopted in France in order to avoid the word *attraction*, which had the taint of Newtonianism. Accordingly we find the word *affinité* employed in the works of French chemists from this time. Thus, in the *Transactions of the French Academy* for 1746, in a paper of Macquer's upon Arsenic, he says[3], 'On peut facilement rendre raison de ces phenomènes par le moyen des affinités que les différens substances qui entrent dans ces combinaisons, ont les uns avec les autres:' and he proceeds to explain the facts by reference to Geoffroy's Table. And in Macquer's *Elements of Chemistry*, which appeared a few years later, the 'Affinity of Composition' is treated of as a leading part of the subject, much in the same way as has been practised in such books up to the present time. From this period, the word appears to have become familiar to all European chemists in the sense of which we are now speaking. Thus, in the year 1758, the Academy of Sciences at Rouen offered a prize for the best dissertation on Affinity. The prize was shared between M. Limbourg of Theux, near Liege, and M. Le Sage

[3] *A. P.* 1746, p. 201.

of Geneva[4]. About the same time other persons (Manherr[5], Nicolai[6], and others) wrote on the same subject, employing the same name.

Nevertheless, in 1775, the Swedish chemist Bergman, pursuing still further this subject of Chemical Affinities, and the expression of them by means of Tables, returned again to the old Newtonian term; and designated the disposition of a body to combine with one rather than another of two others as *Elective Attraction*. And as his work on *Elective Attractions* had great circulation and great influence, this phrase has obtained a footing by the side of *Affinity*, and both one and the other are now in common use among chemists.

4. I have said above that the term *Affinity* is worthy of being retained as a technical term. If we use the word *attraction* in this case, we identify or compare chemical with mechanical attraction; from which identification and comparison, as I have already remarked, no one has yet been able to extract the means of expressing any single scientific truth. If such an identification or comparison be not intended, the use of the same word in two different senses can only lead to confusion; and the proper course, recommended by all the best analogies of scientific history, is to adopt a peculiar term for that peculiar relation on which chemical composition depends. The word *Affinity*, even if it were not rigorously proper according to its common meaning, still, being simple, familiar, and well established in this very usage, is much to be preferred before any other.

But further, there are some analogies drawn from the common meaning of this word, which appear to recommend it as suitable for the office which it has to discharge. For common mechanical attractions and

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[4] Thomson's *Chemistry*, iii. 10. Limbourg's Dissertation was published at Liege, in 1761; and Le Sage's at Geneva.

[5] *Dissertatio de Affinitate Corporum*. Vindob. 1762.

[6] *Progr.* I. II. *de Affinitate Corporum Chimica*. Jen. 1775, 1776.

repulsions, the forces by which one body considered as a *whole* acts upon another external to it, are, as we have said, to be distinguished from those more intimate ties by which the *parts* of each body are held together. Now this difference is implied, if we compare the former relations, the attractions and repulsions, to alliances and wars between States, and the latter, the internal union of particles, to those bonds of affinity which connect the citizens of the same state with one another, and especially to the ties of Family. We have seen that Boerhaave compares the union of two elements of a compound to their marriage; 'we must allow,' says an eminent chemist of our own time[7], 'that there is some truth in this poetical comparison.' It contains this truth,—that the two become one to most intents and purposes, and that the Unit thus formed (the Family) is not a mere juxtaposition of the component parts. And thus the Idea of Affinity as the peculiar principle of chemical composition, is established among chemists, and designated by a familiar and appropriate name.

5. *Analysis is possible.*—We must, however, endeavour to obtain a further insight into this Idea, thus fixed and named. We must endeavour to extricate, if not from the Idea itself, from the processes by which it has obtained acceptation and currency among chemists, some principles which may define its application, some additional specialities in the relations which it implies. This we shall proceed to do.

The Idea of Affinity, as already explained, implies a disposition to combine. But this combination is to be understood as admitting also of a possibility of separation. Synthesis implies Analysis as conceivable: or to recur to the image which we have already used, Divorce is possible when the Marriage has taken place.

That there is this possibility, is a conviction implied in all the researches of chemists, ever since the true notion of composition began to predominate in their investigations. One of the first persons who clearly

[7] Dumas, *Leçons de Phil. Chim.* p. 363.

expressed this conviction was Mayow, an English physician, who published his *Medico-Physical Tracts* in 1674. The first of them *De Sale-Nitro et Spiritu Nitro-Aerio*, contains a clear enunciation of this principle. After showing how, in the combinations of opposite elements, as acid and alkali, their properties entirely disappear, and a new substance is formed not at all resembling either of the ingredients, he adds[8], 'Although these salts thus mixed appear to be destroyed it is still possible for them to be separated from each other, with their powers still entire.' He proceeds to exemplify this, and illustrates it by the same image which I have already already alluded to: 'Salia acida a salibus volatilibus discedunt, ut cum sale fixo tartari, tanquam *sponso* magis idoneo, *conjugium* strictius ineunt.' This idea of a synthesis which left a complete analysis still possible, was opposed to a notion previously current, that when two heterogeneous bodies united together and formed a third body, the two constituents were entirely destroyed, and the result formed out of their ruins[9]. And this conception of Synthesis and Analysis, as processes which are possible successively and alternately, and each of which supposes the possibility of the other, has been the fundamental and regulative principle of the operations and speculations of analytical chemistry from the time of Mayow to the present day.

6. *Affinity is Elective.*—When the idea of chemical affinity, or disposition to unite, was brought into view by the experiments and reasonings of chemists, they found it necessary to consider this disposition as *elective;*—each element *chose* one rather than another of the elements which were presented to it, and quitted its union with one to unite with another which it preferred. This has already appeared in the passage just quoted from Mayow. He adds in the same strain, 'I have no doubt that fixed salts choose one acid rather than another, in order that they may coalesce with it

[8] Cap. xiv. p. 233.
[9] Thomson's *Chemistry*, iii. 8.

in a more intimate union.'—'Nullus dubito salia fixa acidum unum præ aliis *eligere*, ut cum eodem arctiore unione coalescant.' The same thought is expressed and exemplified by other chemists: they notice innumerable cases in which, when an ingredient is combined with a liquid, if a new substance be immersed which has a greater affinity for the liquid, the liquid combines with the new substance by election, and the former ingedient is *precipitated*. Thus Stahl says[10], 'In spirit of nitre dissolve silver; put in copper and the silver is thrown down; put in iron and the copper goes down; put in zinc, the iron precipitates; put in volatile alkali, the zinc is separated; put in fixed alkali, the volatile quits its hold.'—As may be seen in this example, we have in such cases, not only a preference, but a long gradation of preferences. The spirit of nitre will combine with silver, but it prefers copper; prefers iron more; zinc still more; volatile alkali yet more; fixed alkali the most.

The same thing was proved to obtain with regard to each element; and when this was ascertained, it became the object of chemists to express these degrees of preference, by lists in which substances were arranged according to their disposition to unite with another substance. In this manner was formed Geoffroy's Table of Affinities (1718), which we have already mentioned. This Table was further improved by other writers, as Gellert (1751) and Limbourg (1761). Finally Bergman improved these Tables still further, taking into account not only the order of affinities of each element for others, but the *sum* of the tendencies to unite of each two elements, which sum, he held, determined the resulting combination when several elements were in contact with each other.

7. As we have stated in the History[11], when the doctrine of elective affinities had assumed this very definite and systematic form, it was assailed by Berthollet, who maintained, in his *Essai de Statique*

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[10] *Zymotechnia*, 1697, p. 117.

[11] *Hist. Ind. Sc.* b. xiv. c. iii.

Chimique, (1803,) that chemical affinities are *not* elective:—that, when various elements are brought together, their combinations do not depend upon the kind of elements alone, but upon the quantity of each which is present, that which is most abundant always entering most largely into the resulting compounds. It may seem strange that it should be possible, at so late a period of the science, to throw doubt upon a doctrine which had presided over and directed its progress so long. Proust answered Berthollet, and again maintained that chemical affinity is elective. I have, in the History, given the judgment of Berzelius upon this controversy. 'Berthollet,' he says, 'defended himself with an acuteness which makes the reader hesitate in his judgment; but the great mass of facts finally decided the point in favour of Proust.' I may here add the opinion pronounced upon this subject by Dr. Turner[12]: 'Bergman erred in supposing the result of the chemical action to be in every case owing to elective affinity [for this power is modified in its effects by various circumstances]: but Berthollet ran into the opposite extreme in declaring that the effects formerly ascribed to that power are never produced by it. That chemical attraction is exerted between different bodies with different degrees of energy, is, I apprehend, indisputable.' And he then proceeds to give many instances of differences in affinity which cannot be accounted for by the operation of any modifying causes. Still more recently, M. Dumas has taken a review of this controversy; and, speaking with enthusiasm of the work of Berthollet, as one which had been of inestimable service to himself in his early study of chemistry, he appears at first disposed to award to him the victory in this dispute. But his final verdict leaves undamaged the general principle now under our consideration, that chemical affinity is elective. 'For my own part,' he says[13], 'I willingly admit the notions of Berthollet when we have to do with acids or

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[12] *Chemistry*, p. 199. 6th edition.

[13] *Leçons de Philosophie Chimique*, p. 386.

with bases, of which the energy is nearly equal: but when bodies endued with very energetic affinities are in presence of other bodies of which the affinities are very feeble, I propose to adopt the following rule: In a solution, everything remaining dissolved, the strong affinities satisfy themselves, leaving the weak affinities to arrange matters with one another. The strong acids take the strong bases, and the weak acids can only unite with the weak bases. The known facts are perfectly in accordance with this practical rule.' It is obvious that this recognition of a distinction between *strong* and *weak* affinities, which operates to such an extent as to determine entirely the result, is a complete acknowledgement of the Elective nature of Affinity, as far as any person acquainted with chemical operations could contend for it. For it must be allowed by all, that solubility, and other collateral circumstances, influence the course of chemical combinations, since they determine whether or not there shall take place that contact of elements without which affinity cannot possibly operate.

8. *Affinity is Definite as to quantity.*—In proportion as chemists obtained a clearer view of the products of the laboratory as results of the composition of elements, they saw more and more clearly that these results were definite; that one element not only preferred to combine with another of a certain kind, but also would combine with it to a certain extent and no further, thus giving to the result not an accidental and variable, but a fixed and constant character. Thus salts being considered as the result of the combination of two opposite principles, acid and alkali, and being termed *neutral* when these principles exactly balanced each other, Rouelle (who was Royal Professor at Paris in 1742) admits of neutral salts with excess of acid, neutral salts with excess of base, and perfect neutral salts. Beaume maintained[14] against him that there were no salts except those perfectly neutral, the other classes being the results of mixture and imperfect com-

[14] Dumas, *Phil. Chim.* p. 198.

bination. But this question was not adequately treated till chemists made every experiment with the balance in their hands. When this was done, they soon discovered that, in each neutral salt, the proportional weights of the ingredients which composed it were always the same. This was ascertained by Wenzel, whose *Doctrine of the Affinities of Bodies* appeared in 1777. He not only ascertained that the proportions of elements in neutral chemical compounds are definite, but also that they are reciprocal; that is, (to express his results in a manner now employed by chemists), that if A, a certain weight of a certain acid, neutralize *m*, a certain weight of a certain base, and B, a certain weight of a certain other acid, neutralize *n*, a certain weight of a certain other base; the compound of A and *n* will also be neutral; as also that of B and *m*. The same views were again presented by Richter in 1792, in his *Principles of the Measure of Chemical Elements*. And along with these facts, that of the combination of elements in multiple proportions being also taken into account, the foundations of the Atomic Theory were laid; and that Theory was propounded in 1803 by Mr. Dalton. That theory, however, rests upon the Idea of Substance, as well as upon that Idea of Chemical Affinity which we are here considering; and the discussion of its evidence and truth must be for the present deferred.

9. The two principles just explained,—that Affinity is Definite as to the Kind, and as to the Quantity of the elements which it unites,—have here been stated as results of experimental investigation. That they could never have been clearly understood, and therefore never firmly established, without laborious and exact experiments, is certain; but yet we may venture to say that being once fully known, they may seem to thoughtful men to possess an evidence beyond that of mere experiment. For how, in fact, can we conceive combinations, otherwise than as definite in kind and quantity? If we were to suppose each element ready to combine with any other indifferently, and indifferently in any quantity, we should have a world in

which all would be confusion and indefiniteness. There would be no fixed kinds of bodies. Salts, and stones, and ores, would approach to and graduate into each other by insensible degrees. Instead of this, we know that the world consists of bodies distinguishable from each other by definite differences, capable of being classified and named, and of having general propositions asserted concerning them. And as we cannot conceive a world in which this should not be the case, it would appear that we cannot conceive a state of things in which the laws of the combination of elements should not be of that definite and measured kind which we have above asserted.

This will, perhaps, appear more clearly by stating our fundamental convictions respecting chemical composition in another form, which I shall, therefore, proceed to do.

10. *Chemical Composition determines Physical Properties.*—However obscure and incomplete may be our conception of the internal powers by which the ultimate particles of bodies are held together, it involves, at least, this conviction:—that these powers are what determine bodies to *be* bodies, and therefore contain the reason of all the properties which, as bodies, they possess. The forces by which the particles of a body are held together, also cause it to be hard or soft, heavy or light, opake or transparent, black or red; for if these forces are not the cause of these peculiarities, what can be the cause? By the very supposition which we make respecting these forces, they include all the relations by which the parts are combined into a whole, and therefore they, and they only, must determine all the attributes of the whole. The foundation of all our speculations respecting the intimate constitution of bodies must be this principle, that their composition determines their properties.

Accordingly we find our chemists reasoning from this principle with great confidence, even in doubtful cases. Thus Davy, in his researches concerning the diamond, says: 'That some chemical difference must exist between the hardest and most beautiful of the

gems and charcoal, between a non-conductor and a conductor of electricity, it is scarcely possible to doubt: and it seems reasonable to expect that a very refined or perfect chemistry will confirm *the analogies of nature;* and show that bodies cannot be the same in their composition or chemical nature, and yet totally different in their chemical properties.' It is obvious that the principle here assumed is so far from being a mere result of experience, that it is here appealed to to prove that all previous results of experience on this subject must be incomplete and inaccurate; and that there must be some chemical difference between charcoal and diamond, though none had hitherto been detected.

11. In what manner, according to what rule, the chemical composition shall determine the kind of the substance, we cannot reasonably expect to determine by mere conjecture or assumption, without a studious examination of natural bodies and artificial compounds. Yet even in the most recent times, and among men of science, we find that an assumption of the most arbitrary character has in one case been mixed up with this indisputable principle, that the elementary composition determines the kind of the substance. In the classification of minerals, one school of mineralogists have rightly taken it as their fundamental principle that the chemical composition shall decide the position of the mineral in the system. But they have appended to this principle, arbitrarily and unjustifiably, the maxim that the element which is *largest in quantity* shall fix the class of the substance. To make such an assumption is to renounce, at once, all hope of framing a system which shall be governed by the resemblances of the things classified; for how can we possibly know beforehand that fifty-five per cent. of iron shall give a substance its predominant properties, and that forty-five per cent. shall not? Accordingly, the systems of mineralogical arrangement which have been attempted in this way, (those of Haüy, Phillips, and others,) have been found inconsistent with themselves, ambiguous, and incapable of leading to any general truths.

12. *Chemical Composition and Crystalline Form correspond.*—Thus the physical properties of bodies depend upon their chemical composition, but in a manner which a general examination of bodies with reference to their properties and their composition can alone determine. We may, however, venture to assert further, that the more definite the properties are, the more distinct may we expect to find this dependence. Now the most definite of the properties of bodies are those constant properties which involve relations of space; that is, their figure. We speak not, however, of that external figure, derived from external circumstances, which, so far from being constant and definite, is altogether casual and arbitrary; but of that figure which arises from their internal texture, and which shows itself not only in the regular forms which they spontaneously assume, but in the disposition of the parts to separate in definite directions, and no others. In short, the most definite of the properties of perfect chemical compounds is their *crystalline structure;* and therefore it is evident that the crystalline structure of each body, and the forms which it affects, must be in a most intimate dependence upon its chemical composition.

Here again we are led to the brink of another theory;—that of crystalline structure, which has excited great interest among philosophers ever since the time of Haüy. But this theory involves, besides that idea of chemical composition with which we are here concerned, other conceptions, which enter into the relations of figure. These conceptions, governed principally by the Idea of Symmetry, must be unfolded and examined before we can venture to discuss any theory of crystallization: and we shall proceed to do this as soon as we have first duly considered the Idea of Substance and its consequences.

CHAPTER III.

OF THE IDEA OF SUBSTANCE.

1. *Axiom of the Indestructibility of Substance.*—WE now come to an Idea of which the history is very different from those of which we have lately been speaking. Instead of being gradually and recently brought into a clear light, as has been the case with the Ideas of Polarity and Affinity, the Idea of Substance has been entertained in a distinct form from the first periods of European speculation. That this is so, is proved by our finding a principle depending upon this Idea current as an axiom among the early philosophers of Greece:—namely, that *nothing can be produced out of nothing.* Such an axiom, more fully stated, amounts to this: that the substance of which a body consists is incapable of being diminished (and consequently incapable of being augmented) in quantity, whatever apparent changes it may undergo. Its forms, its distribution, its qualities, may vary, but the substance itself is identically the same under all these variations.

The axiom just spoken of was the great principle of the physical philosophy of the Epicurean school, as it must be of every merely material philosophy. The reader of Lucretius will recollect the emphasis with which it is repeatedly asserted in his poem:

> E nilo nil gigni, in nilum nil posse reverti;
> Nought comes of nought, nor ought returns to nought.

Those who engaged in these early attempts at physical speculation were naturally much pleased with the clearness which was given to their notions of change, composition, and decomposition, by keeping steadily hold of the Idea of Substance, as marked by this

fundamental axiom. Nor has its authority ever ceased to be acknowledged. A philosopher was asked[1], What is the weight of smoke? He answered, 'Subtract the weight of the ashes from the weight of the wood which is burnt, and you have the weight of the smoke.' This reply would be assented to by all; and it assumes as incontestable that even under the action of fire, the material, the substance, does not perish, but only changes its form.

This principle of the indestructibility of substance might easily be traced in many reasonings and researches, ancient and modern. For instance, when the chemist works with the *retort*, he places the body on which he operates in one part of an inclosed cavity, which, by its bendings and communications, separates at the same time that it confines, the products which result from the action of fire: and he assumes that this process is an analysis of the body into its ingredients, not a creation of anything which did not exist before, or a destruction of anything which previously existed. And he assumes further, that the total quantity of the substance thus analysed is the sum of the quantities of its ingredients. This principle is the very basis of chemical speculation, as we shall hereafter explain more fully.

2. *The Idea of Substance.*—The axiom above spoken of depends upon the Idea of Substance, which is involved in all our views of external objects. We unavoidably assume that the qualities and properties which we observe are properties of *things;*—that the adjective implies a substantive;—that there is, besides the external characters of things, something *of which* they are the characters. An apple which is red, and round, and hard, is not merely redness, and roundness, and hardness: these circumstances may all alter while the apple remains the same apple. Behind or under the appearances which we see, we conceive something of which we think; or, to use the metaphor which obtained currency among the ancient philosophers, the

[1] Kant, *Kritik der R. V.* p. 167.

attributes and qualities which we observe are supported by and inherent in something: and this something is hence called a *substratum* or *substance*,—that which stands *beneath* the apparent qualities and supports them.

That we have such an *Idea*, using the term 'Idea' in the sense in which I have employed it throughout these disquisitions, is evident from what has been already said. The Axiom of the Indestructibility of Substance proves the existence of the Idea of Substance, just as the Axioms of Geometry and Arithmetic prove the existence of the Ideas of Space and Number. In the case of Substance, as of space or number, the ideas cannot be said to be borrowed from experience, for the axioms have an authority of a far more comprehensive and demonstrative character than any which experience can bestow. The axiom that nothing can be produced from nothing and nothing destroyed, is so far from being a result of experience, that it is apparently contradicted by the most obvious observation. It has, at first, the air of a paradox; and by those who refer to it, it is familiarly employed to show how fallacious common observation is. The assertion is usually made in this form;—that nothing is created and nothing annihilated, *notwithstanding* that the common course of our experience appears to show the contrary. The principle is not an empirical, but a necessary and universal truth;—is collected, not from the evidence of our senses, but from the operation of our ideas. And thus the universal and undisputed authority of the axiom proves the existence of the Idea of Substance.

3. *Locke's Denial of the Idea of Substance.*—I shall not attempt to review the various opinions which have been promulgated respecting this Idea: but it may be worth our while to notice briefly the part which it played in the great controversy concerning the origin of our ideas which Locke's *Essay* occasioned. Locke's object was to disprove the existence of all ideas not derived from Sensation or Reflection: and since the idea of substance as distinct from external qualities, is

manifestly not derived directly from sensation, nor by any very obvious or distinct process from reflection, Locke was disposed to exclude the idea as much as possible. Accordingly, in his argumentation against Innate Ideas[2], he says plainly, 'the idea of substance, which we neither have nor can have by sensation or reflection.' And the inference which he draws is, 'that we have no such clear idea at all.' What then, it may be asked, do we mean by the word *substance?* This also he answers, though somewhat strangely, 'We signify nothing by the word *substance*, but only an uncertain supposition of we know not what, *i. e.* of something whereof we have no particular distinct positive idea, which we take to be the substratum, or support, of those ideas we know.' That while he indulged in this tautological assertion of our ignorance and uncertainty, he should still have been compelled to acknowledge that the word substance had some meaning, and should have been driven to explain it by the identical metaphors of 'substratum' and 'support,' is a curious proof how impossible it is entirely to reject this idea.

But as we have already seen, the supposition of the existence of substance is so far from being uncertain, that it carries with it irresistible conviction, and substance is necessarily conceived as something which cannot be produced or destroyed. It may be easily supposed, therefore, that when the controversy between Locke and his assailants came to this point, he would be in some difficulty. And, indeed, though with his accustomed skill in controversy, he managed to retain a triumphant tone, he was driven from his main points. Thus he repels the charge that he took the being of substance to be doubtful[3]. He says, 'Having everywhere affirmed and built upon it that man is a substance, I cannot be supposed to question or doubt of the being of substance, till I can question or doubt of my own being.' He attempts to make a stand by saying that *being* of things does not depend upon our

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[2] *Essay*, b. i. c. iv. s. 18.

[3] *Essay*, b. ii. c. ii. and *First Letter to the Bishop of Worcester.*

ideas; but if he had been asked how, without having an *idea* of substance, he *knew* substance *to be*, it is difficult to conceive what answer he could have made. Again, he had said that our idea of substance arises from our 'accustoming ourselves to suppose' a substratum of qualities. Upon this his adversary, Bishop Stillingfleet, very properly asks, Is this custom grounded upon true reason or no? To which Locke replies, that it is grounded upon this: That we cannot conceive how simple ideas of sensible qualities should subsist alone; and therefore we suppose them to exist in, and to be supported by some common subject, which support we denote by the name substance. Thus he allows, not only that we necessarily assume the reality of substance, but that we cannot conceive qualities without substance; which are concessions so ample as almost to include all that any advocate for the Idea of Substance need desire.

Perhaps Locke, and the adherents of Locke, in denying that we have an idea of substance in general, were latently influenced by finding that they could not, by any effort of mind, call up any *image* which could be considered as an image of substance in general. That in this sense we have no idea of substance, is plain enough; but in the same sense we have no idea of space in general, or of time, or number, or cause, or resemblance. Yet we certainly have such a power of representing to our minds space, time, number, cause, resemblance, as to arrive at numerous truths by means of such representations. These general representations I have all along called Ideas, nor can I discover any more appropriate word; and in this sense, we have also, as has now been shown, an Idea of Substance.

4. *Is all Material Substance heavy?*—The principle that the quantity of the substance of any body remains unchanged by our operations upon it, is, as we have said, of universal validity. But then the question occurs, how are we to ascertain the quantity of substance, and thus, to apply the principle in particular cases. In the case above mentioned, where

smoke was to be weighed, it was manifestly assumed that the quantity of the substance might be known by its weight; and that the total quantity being unchanged, the total weight also would remain the same. Now on what grounds do we make this assumption? Is all material substance heavy? and if we can assert this to be so, on what grounds does the truth of the assertion rest? These are not idle questions of barren curiosity; for in the history of that science (Chemistry) to which the Idea of Substance is principally applicable, nothing less than the fate of a comprehensive and long established theory (the Phlogiston theory) depended upon the decision of this question. When it was urged that the reduction of a metal from a calcined to a metallic form could not consist in the *addition* of phlogiston, because the metal was lighter than the calx had been; it was replied by some, that this was not conclusive, for that phlogiston was a principle of levity, diminishing the weight of the body to which it was added. This reply was, however, rejected by all the sounder philosophers, and the force of the argument finally acknowledged. But why was this suggestion of a substance having no weight, or having absolute levity, repudiated by the most reflective reasoners? It is assumed, it appears, that all matter must be heavy; what is the ground of this assumption?

The ground of such an assumption appears to be the following. Our idea of substance includes in it this:—that substance is a quantity capable of addition; and thus capable of making up, by composition, a sum equal to all its parts. But substance, and the quantity of substance, can be known to us only by its attributes and qualities. And the qualities which are capable constantly and indefinitely of increase and diminution by increase and diminution of the parts, must be conceived inseparable from the substance. For the qualities, if removable from the substance at all, must be removable by some operation performed upon the substance; and by the idea of substance, all such operations are only equivalent to separation, junction, and union of parts. Hence those characters

which thus universally increase and diminish by addition and subtraction of the things themselves, belong to the substance of the things. They are measures of its quantity, and are not merely its separable qualities.

The weight of bodies is such a character. However we compound or divide bodies, we compound and divide their weight in the same manner. We may dismember a body into the minutest parts; but the sum of the weights of the parts is always equal to the whole weight of the body. The weight of a body can be in no way increased or diminished, except by adding something to it or taking something from it. If we bake a brick, we do not conceive that the change of colour or of hardness, implies that anything has been created or destroyed. It may easily be that the parts have only assumed a new arrangement; but if the brick have lost weight, we suppose that something (moisture for instance) has been removed elsewhere.

Thus weight is apprehended as essential to matter. In considering the dismemberment or analysis of bodies, we assume that there must be some criterion of the quantity of substance; and this criterion can possess no other properties than their weight possesses. If we assume an element which has no weight, or the weight of which is negative, as some of the defenders of phlogiston attempted to do, we put an end to all speculation on such subjects. For if weight is not the criterion of the quantity of one element, phlogiston for instance, why is weight the criterion of the quantity of any other element? We may, by the same right, assume any other real or imaginary element to have levity instead of gravity; or to have a peculiar intensity of gravity which makes its weight no index of its quantity. In short, if we do this, we deprive of all possibility of application our notions of element, analysis, and composition; and violate the postulates on which the questions are propounded which we thus attempt to decide.

We must, then, take a constant and quantitative property of matter, such as weight is, to be an index

of the quantity of matter or of substance to which it belongs. I do not here speak of the question which has sometimes been proposed, whether the *weight* or the *inertia* of bodies be the more proper measure of the quantity of matter. For the measure of inertia is regulated by the same assumption as that of substance:—that the quantity of the whole must be equal to the quantity of all the parts: and inertia is measured by weight, for the same reason that substance is so.

Having thus established the certainty, and ascertained the interpretation of the fundamental principle which the Idea of Substance involves, we are prepared to consider its application in the science upon which it has a peculiar bearing.

NOTE TO CHAPTER III.

[3rd Ed.]—[The doctrine here propounded, that All Matter is Heavy, has been opposed by Sir William Hamilton of Edinburgh. (*Works of Reid*, note, p. 853.) This writer is a man of unquestionable acuteness and of very extensive reading; but his acuteness shows itself in barren ontological distinctions, which appear to me to be of the same character as the speculations of the eminent Schoolmen of the most sterile periods of the dark ages. That he should have no conception of progressive or inductive science is not wonderful, when we recollect that he holds, as an important part of his philosophy, that the study of mathematics perverts and obscures the mind. But it may be of some interest to consider his objections to the doctrine here maintained.

He says, 1st, that our reasoning assumes that we must necessarily have it in our power to ascertain the Quantity of Matter; whereas this may be a problem out of the reach of human determination.

To this I reply, that my reasoning *does* assume that there is a science, or sciences, which make assertions concerning the Quantity of Matter: Mechanics and Chemistry are such sciences. My assertion is, that to make such sciences possible, Quantity of Matter must be proportional to Weight. If my opponent deny that Mechanics and Chemistry can exist as sciences, he may invalidate my proof; but not otherwise.

2. He says that there are two conceivable ways of estimating the Quantity of Matter: by the Space occupied, and by the Weight or Inertia; and that I assume the second measure gratuitously.

To which I reply, that the most elementary steps in Mechanics and in Chemistry contradict the notion that

the Quantity of Matter is proportionate to the Space. They proceed necessarily on a distinction between Space and Matter:—between mere Extension and material Substance.

3. He allows that we cannot make the Extension of a body the measure of the Quantity of Matter, because, he says, we do not know if 'the compressing force' is such as to produce 'the closest compression.' That is, he assumes a compressing force, assumes a closest compression, assumes a peculiar (and very improbable) atomic hypothesis; and all this to supply a reason why we are not to believe the first simple principle of Mechanics and Chemistry.

4. He speaks of 'a series of apparent fluids (as Light or its vehicle, the Calorific, the Electro-galvanic, and Magnetic agents) which we can neither denude of their character of substance, nor clothe with the attribute of weight.'

To which my reply is, that precisely because I cannot 'clothe' these agents with the attribute of Weight, I *do* 'denude them of the character of Substance.' They are not substances, but agencies. These Imponderable Agents are not properly called 'Imponderable Fluids.' This I conceive that I have proved; and the proof is not shaken by denying the conclusion without showing any defect in the reasoning.

5. Finally, my critic speaks about 'a logical canon,' and about 'a criterion of truth, subjectively necessary and objectively certain;' which matters I shall not waste the reader's time by discussing.]

CHAPTER IV.

Application of the Idea of Substance in Chemistry.

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1. *A Body is Equal to the Sum of its Elements.*—From the earliest periods of chemistry the balance has been familiarly used to determine the proportions of the ingredients and of the compound; and soon after the middle of the last century, this practice was so studiously followed, that Wenzel and Richter were thereby led to the doctrine of Definite Proportions. But yet the full value and significance of the balance, as an indispensable instrument in chemical researches, was not understood till the gaseous, as well as solid and fluid ingredients were taken into the account. When this was done, it was found that the principle, that the whole is equal to the sum of its parts, of which, as we have seen, the necessary truth, in such cases, flows from the idea of substance, could be applied in the most rigorous manner. And conversely, it was found that by the use of the balance, the chemist could decide, in doubtful cases, which was a whole, and which were parts.

For chemistry considers all the changes which belong to her province as compositions and decompositions of elements; but still the question may occur, whether an observed change be the one or the other. How can we distinguish whether the process which we contemplate be composition or decomposition?—whether the new body be formed by addition of a new, or subtraction of an old element? Again; in the case of decomposition, we may inquire, What are the ultimate limits of our analysis? If we decompound bodies into others more and more simple, how far can we carry this succession

of processes? How far can we proceed in the road of analysis? And in our actual course, what evidence have we that our progress, as far as it has gone, has carried us from the more complex to the more simple?

To this we reply, that the criterion which enables us to distinguish, decidedly and finally, whether our process have been a mere analysis of the proposed body into its ingredients, or a synthesis of some of them with some new element, is the principle stated above, that the weight of the whole is equal to the weight of all the parts. And no process of chemical analysis or synthesis can be considered complete till it has been verified by this fact;—by finding that the weight of the compound is the weight of its supposed ingredients; or, that if there be an element which we think we have detached from the whole, its loss is betrayed by a corresponding diminution of weight.

I have already noticed what an important part this principle has played in the great chemical controversy which ended in the establishment of the oxygen theory. The calcination of a metal was decided to be the union of oxygen with the metal, and not the separation of phlogiston from it, because it was found that in the process of calcination, the weight of the metal increased, and increased exactly as much as the weight of ambient air diminished. When oxygen and hydrogen were exploded together, and a small quantity of water was produced, it was held that this was really a synthesis of water, because, when very great care was taken with the process, the weight of the water which resulted was equal to the weight of the gases which disappeared.

2. *Lavoisier.*—It was when gases came to be considered as entering largely into the composition of liquid and solid bodies, that extreme accuracy in weighing was seen to be so necessary to the true understanding of chemical processes. It was in this manner discovered by Lavoisier and his contemporaries that oxygen constitutes a large ingredient of calcined metals, of acids, and of water. A countryman of Lavoisier[1]

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[1] M. Dumas, *Leçons de la Philosophie Chimique.* 1837. p. 157.

has not only given most just praise to that great philosopher for having constantly tested all his processes by a careful and skilful use of the balance, but has also claimed for him the merit of having introduced the maxim, that in chemical operations nothing is created and nothing lost. But I think it is impossible to deny that this maxim is assumed in all the attempts at analysis made by his contemporaries, as well as by him. This maxim is indeed included in any clear notion of analysis: it could not be the result of the researches of any one chemist, but was the governing principle of the reasonings of all. Lavoisier, however, employed this principle with peculiar assiduity and skill. In applying it, he does not confine himself to mere additions and subtractions of the quantities of ingredients; but often obtains his results by more complex processes. In one of his investigations he says, 'I may consider the ingredients which are brought together, and the result which is obtained as an algebraical equation; and if I successively suppose each of the quantities of this equation to be unknown, I can obtain its value from the rest: and thus I can rectify the experiment by the calculation, and the calculation by the experiment. I have often taken advantage of this method, in order to correct the first results of my experiments, and to direct me in repeating them with proper precautions.'

The maxim, that the whole is equal to the sum of all its parts, is thus capable of most important and varied employment in chemistry. But it may be applied in another form to the exclusion of a class of speculations which are often put forwards.

3. *Maxim respecting Imponderable Elements.*—Several of the phenomena which belong to bodies, as heat, light, electricity, magnetism, have been explained hypothetically by assuming the existence of certain fluids; but these fluids have never been shown to have weight. Hence such hypothetical fluids have been termed *imponderable elements.* It is however plain, that so long as these fluids appear to be without weight, they are not *elements* of bodies in the same

sense as those elements of which we have hitherto been speaking. Indeed we may with good reason doubt whether those phenomena depend upon transferable fluids at all. We have seen strong reason to believe that light is not matter, but only motion; and the same thing appears to be probable with regard to heat. Nor is it at all inconceivable that a similar hypothesis respecting electricity and magnetism should hereafter be found tenable. Now if heat, light, and those other agents, be not matter, they are not *elements* in such a sense as to be included in the principle referred to above, That the body is equal to the sum of its elements. Consequently the maxim just stated, that in chemical operations nothing is created, nothing annihilated, does not apply to Light and Heat. They are not *things*. And whether heat can be produced where there was no heat before, and light struck out from darkness, the ideas of which we are at present treating do not enable us to say. In reasoning respecting chemical synthesis and analysis therefore, we shall only make confusion by attempting to include in our conception the Light and Heat which are produced and destroyed. Such phenomena may be very proper subjects of study, as indeed they undoubtedly are; but they cannot be studied to advantage by considering them as sharing the nature of composition and decomposition.

Again: in all attempts to explain the processes of nature, the proper course is, first to measure the facts with precision, and then to endeavour to understand their cause. Now the facts of chemical composition and decomposition, the weights of the ingredients and of the compounds, are facts measurable with the utmost precision and certainty. But it is far otherwise with the light and heat which accompany chemical processes. When combustion, deflagration, explosion, takes place, how can we measure the light or the heat? Even in cases of more tranquil action, though we can apply the thermometer, what does the thermometer tell us respecting the *quantity* of the heat? Since then we have no measure which is of any value as

regards such circumstances in chemical changes, if we attempt to account for these phenomena *on chemical principles*, we introduce, into investigations in themselves perfectly precise and mathematically rigorous, another class of reasonings, vague and insecure, of which the only possible effect is to vitiate the whole reasoning, and to make our conclusions inevitably erroneous.

We are led then to this maxim: that *imponderable fluids are* not *to be admitted as chemical elements of bodies*[2].

4. It appears, I think, that our best and most philosophical chemists have proceeded upon this principle in their investigations. In reasoning concerning the constitution of bodies and the interpretation of chemical changes, the attempts to include in these interpretations the heat or cold produced, by the addition or subtraction of a certain hypothetical 'caloric,' have become more and more rare among men of science. Such statements, and the explanations often put forwards of the light and heat which appear under various circumstances in the form of fire, must be considered as unessential parts of any sound theory. Accordingly we find Mr. Faraday gradually relinquishing such views. In January, 1834, he speaks generally of an hypothesis of this kind[3]: 'I cannot refrain from recalling here the beautiful idea put forth, I believe by Berzelius, in his development of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination

[2] See the answer to Sir William Hamilton's objections, at the end of the last chapter.

Since we are thus warned by a sound view of the nature of science, from considering chemical affinity as having any hold upon imponderable elements, we are manifestly still more decisively prohibited from supposing mechanical impulse or pressure to have any effect upon such elements. To make this supposition, is to connect the most subtle and incorporeal objects which we know in nature by the most gross material ties. This remark seems to be applicable to M. Poisson's hypothesis that the electric fluid is retained at the surface of bodies by the pressure of the atmosphere.

[3] *Researches*, 870.

are the consequence of the electric discharge which is at that moment taking place.' But in April of the same year[4], he observes, that in the combination of oxygen and hydrogen to produce water, electric powers to a most enormous amount are for the time active, but that the flame which is produced gives but feeble traces of such powers. 'Such phenomena,' therefore, he adds, 'may not, cannot, be taken as evidences of the nature of the action; but are merely incidental results, incomparably small in relation to the forces concerned, and supplying no information of the way in which the particles are active on each other, or in which their forces are finally arranged.'

In pursuance of this maxim, we must consider as an unessential part of the oxygen theory that portion of it, much insisted upon by its author at the time, in which when sulphur, for instance, combined with oxygen to produce sulphuric acid, the combustion was accounted for by means of the *caloric* which was supposed to be *liberated* from its combination with oxygen.

5. *Controversy of the Composition of Water.*—There is another controversy of our times to which we may with great propriety apply the maxim now before us. After the glory of having first given a true view of the composition of water had long rested tranquilly upon the names of Cavendish and Lavoisier, a claim was made in favour of James Watt as the real author of this discovery by his son, (Mr J. Watt,) and his eulogist, (M. Arago[5]). It is not to our purpose here to discuss the various questions which have arisen on this subject respecting priority of publication, and respecting the translation of opinions published at one time into the language of another period. But if we look at Watt's own statement of his views, given soon after those of Cavendish had been published, we shall perceive that it is marked by a violation of this maxim: we shall find that he does admit imponderable fluids

[4] *Researches*, 960.

[5] Éloge de James Watt, *Annuaire du Bur. des Long.* 1839.

as chemical elements; and thus shows a vagueness and confusion in his idea of chemical composition. With such imperfection in his views, it is not surprising that Watt, not only did not anticipate, but did not apprehend quite precisely the discovery of Cavendish and Lavoisier. Watt's statement of his views is as follows[6]:—'Are we not authorized to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light; and that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be only a modification of heat, or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston and united to elementary heat?'

When we compare this doubtful and hypothetical statement, involving so much that is extraneous and heterogeneous, with the conclusion of Cavendish, in which there is nothing hypothetical or superfluous, we may confidently assent to the decision which has been pronounced by one[7] of our own time in favour of Cavendish. And we may with pleasure recognize, in this enlightened umpire, a due appreciation of the value of the maxim on which we are now insisting. 'Cavendish,' says Mr. Vernon Harcourt, 'pared off

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[6] *Phil. Trans.* 1784, p. 332.

[7] The Rev. W. Vernon Harcourt, Address to the British Association, 1839.—Since the first edition of this work was published, and also since the second edition of the *History of the Inductive Sciences*, Mr. Watt's correspondence bearing upon the question of the Composition of Water has been published by Mr. Muirhead I do not find, in this publication, any reason for withdrawing what I have stated in the text above: but with reference to the statement in the *History*, it appears that Mr. Cavendish's claim to the discovery was not uncontested in his own time. Mr. Watt had looked at the composition of water, as a problem to be solved, perhaps more distinctly than Mr. Cavendish had done; and he conceived himself wronged by Mr. Cavendish's putting forwards his experiment as the first solution of this problem.

from the hypotheses their theories of combustion, and *affinities of imponderable for ponderable matter*, as complicating chemical with physical considerations.'

6. *Relation of Heat to Chemistry.*—But while we thus condemn the attempts to explain the thermotical phenomena of chemical processes by means of chemical considerations, it may be asked if we are altogether to renounce the hope of understanding such phenomena? It is plain, it may be said, that heat generated in chemical changes is always a very important circumstance, and can sometimes be measured, and perhaps reduced to laws; are we prohibited from speculating concerning the causes of such circumstances and such laws? And to this we reply, that we may properly attempt to connect chemical with thermotical processes, *so far as* we have obtained a clear and probable view of the nature of the thermotical processes. When our theory of Thermotics is tolerably complete and certain, we may with propriety undertake to connect it with our theory of Chemistry. But at present we are not far enough advanced in our knowledge of heat to make this attempt with any hope of success. We can hardly expect to understand the part which heat plays in the union of two bodies, when we cannot as yet comprehend in what manner it produces the liquefaction or vaporization of one body. We cannot look to account for Gay Lussac and Dalton's Law, that all gases expand equally by heat, till we learn how heat causes a gas to expand. We cannot hope to see the grounds of Dulong and Petit's Law, that the specific heat of all atoms is the same, till we know much more, not only about atoms, but about specific heat. We have as yet no thermotical theory which even professes to account for all the prominent facts of the subject[8]: and the theories which have been proposed are of the most diverse kind. Laplace assumes particles of bodies surrounded by atmospheres of caloric[9]; Cauchy makes heat consist in longitudinal vibrations of the ether of which transverse vibrations

[8] *Hist. Ind. Sci.* b. x. c. 4.

[9] *Ib.*

produce light: in Ampère's theory[10], heat consists in the vibrations of the particles of bodies. And so long as we have nothing more certain in our conceptions of heat than the alternative of these and other precarious hypotheses, how can we expect to arrive at any real knowledge, by connecting the results of such hypotheses with the speculations of Chemistry, of which science the theory is at least equally obscure?

The largest attempts at chemical theory have been made in the form of the Atomic Theory, to which I have just had occasion to allude. I must, therefore, before quitting the subject, say a few words respecting this theory.

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[10] *Hist. Ind. Sci.* b. x. c. 4.

CHAPTER V.

The Atomic Theory.

1. *The Atomic Theory considered on Chemical Grounds.*—We have already seen that the combinations which result from chemical affinity are definite, a certain quantity of one ingredient uniting, not with an uncertain, but with a certain quantity of another ingredient. But it was found, in addition to this principle, that one ingredient would often unite with another in different proportions, and that, in such cases, these proportions are multiples one of another. In the three salts formed by potassa with oxalic acid, the quantities of acid which combine with the same quantity of alkali are exactly in the proportion of the numbers 1, 2, 4. And the same rule of the existence of multiple proportions is found to obtain in other cases.

It is obvious that such results will be accounted for, if we suppose that the base and the acid consist each of numerous definite equal particles, and that the formation of the salts above mentioned consists in the combination of one particle of the base with one particle of acid, with two particles of acid, and with four particles of acid, respectively. But further; as we have already stated, chemical affinity is not only definite, but reciprocal. The proportions of potassa and soda which form neutral salts being 590 and 391 in one case, they are so in all cases. These numbers represent the *proportions* of weight in which the two bases, potassa and soda, enter into analogous combinations; 590 of potassa is *equivalent* to 391 of soda. These facts with regard to combination are still expressed by the above supposition of equal particles, assuming that the weights of a

particle of potassa and of soda are in the proportion of 590 to 391.

But we pursue our analysis further. We find that potassa is a compound of a metallic base, potassium, and of oxygen, in the proportion of 490 to 100; we suppose, then, that the particle of potassa consists of a particle of potassium and a particle of oxygen; and these latter particles, since we see no present need to suppose them divided, potassium and oxygen being simple bodies, we may call *atoms*, and assume to be indivisible. And by supposing all simple bodies to consist of such atoms, and compounds to be formed by the union of two, or three, or more of such atoms, we explain the occurrence of definite and multiple proportions, and we construct the Atomic Theory.

2. *Hypothesis of Atoms.*—So far as the assumption of such atoms as we have spoken of serves to express those laws of chemical composition which we have referred to, it is a clear and useful generalization. But if the Atomic Theory be put forwards (and its author, Dr. Dalton, appears to have put it forwards with such an intention,) as asserting that chemical elements are really composed of *atoms*, that is, of such particles not further divisible, we cannot avoid remarking, that for such a conclusion, chemical research has not afforded, nor can afford, any satisfactory evidence whatever. The smallest observable quantities of ingredients, as well as the largest, combine according to the laws of proportions and equivalence which have been cited above. How are we to deduce from such facts any inference with regard to the existence of certain smallest possible particles? The Theory, when dogmatically taught as a physical truth, asserts that all observable quantities of elements *are* composed of proportional numbers of particles which can no further be subdivided; but all which observation teaches us is, that *if* there be such particles, they are smaller than the smallest observable quantities. In chemical experiment, at least, there is not the slightest positive evidence for the existence of such atoms. The assumption of *indivisible* particles, smaller than the smallest

observable, which combine, particle with particle, will explain the phenomena; but the assumption of particles bearing this proportion, but *not* possessing the property of indivisibility, will explain the phenomena at least equally well. The decision of the question, therefore, whether the Atomic Hypothesis be the proper way of conceiving the chemical combinations of substances, must depend, not upon chemical facts, but upon our conception of Substance. In this sense the question is an ancient and curious controversy, and we shall hereafter have to make some remarks upon it.

3. *Chemical Difficulties of the Hypothesis.*—But before doing this, we may observe that there is no small difficulty in reconciling this hypothesis with the facts of chemistry. According to the theory, all salts, compounded of an acid and a base, are analogous in their atomic constitution; and the number of atoms in one such compound being known or assumed, the number of atoms in other salts may be determined. But when we proceed in this course of reasoning to other bodies, as metals, we find ourselves involved in difficulties. The protoxide of iron is a base which, according to all analogy, must consist of one atom of iron and one of oxygen: but the peroxide of iron is also a base, and it appears by the analysis of this substance that it must consist of *two-thirds* of an atom of iron and one atom of oxygen. Here, then, our indivisible atoms must be divisible, even upon chemical grounds. And if we attempt to evade this difficulty by making the peroxide of iron consist of two atoms of iron and three of oxygen, we have to make a corresponding alteration in the theoretical constitution of all bodies analogous to the protoxide; and thus we overturn the very foundation of the theory. Chemical facts, therefore, not only do not prove the Atomic Theory as a physical truth, but they are not, according to any modification yet devised of the theory, reconcileable with its scheme.

Nearly the same conclusions result from the attempts to employ the Atomic Hypothesis in expressing another important chemical law;—the law of the

combinations of gases according to definite proportions of their volumes, experimentally established by Gay Lussac[1]. In order to account for this law, it has been very plausibly suggested that all gases, under the same pressure, contain an equal number of atoms in the same space; and that when they combine, they unite atom to atom. Thus one volume of chlorine unites with one volume of hydrogen, and forms hydrochloric acid[2]. But then this hydrochloric acid occupies the space of the two volumes; and therefore the proper number of particles cannot be supplied, and the uniform distribution of atoms in all gases maintained, without dividing into two each of the compound particles, constituted of an atom of chlorine and an atom of hydrogen. And thus in this case, also, the Atomic Theory becomes untenable if it be understood to imply the indivisibility of the atoms.

In all these attempts to obtain a distinct physical conception of chemical union by the aid of the Atomic Hypothesis, the atoms are conceived to be associated by certain forces of the nature of mechanical attractions. But we have already seen[3] that no such mode of conception can at all explain or express the facts of chemical combination; and therefore it is not wonderful that when the Atomic Theory attempts to give an account of chemical relations by contemplating them under such an aspect, the facts on which it grounds itself should be found not to authorize its positive doctrines; and that when these doctrines are tried upon the general range of chemical observation, they should prove incapable of even expressing, without self-contradiction, the laws of phenomena.

4. *Grounds of the Atomic Doctrine.*—Yet the doctrine of atoms, or of substance as composed of indivisible particles, has in all ages had great hold upon the minds of physical speculators; nor would this doctrine ever have suggested itself so readily, or have been maintained so tenaciously, as the true mode of

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[1] *Hist. Ind. Sc.* b. xiv. c. 8. [2] Dumas, *Phil. Chim.* 263.
[3] See Chapter I. of this book.

conceiving chemical combinations, if it had not been already familiar to the minds of those who endeavour to obtain a general view of the constitution of nature. The grounds of the assumption of the atomic structure of substance are to be found rather in the idea of substance itself, than in the experimental laws of chemical affinity. And the question of the existence of atoms, thus depending upon an idea which has been the subject of contemplation from the very infancy of philosophy, has been discussed in all ages with interest and ingenuity. On this very account it is unlikely that the question, so far as it bears upon chemistry, should admit of any clear and final solution. Still it will be instructive to look back at some of the opinions which have been delivered respecting this doctrine.

5. *Ancient Prevalence of the Atomic Doctrine.*—The doctrine that matter consists of minute, simple, indivisible, indestructible particles as its ultimate elements, has been current in all ages and countries, whenever the tendency of man to wide and subtle speculations has been active. I need not attempt to trace the history of this opinion in the schools of Greece and Italy. It was the leading feature in the physical tenets of the Epicureans, and was adopted by their Roman disciples, as the poem of Lucretius copiously shows us. The same tenet had been held at still earlier periods, in forms more or less definite, by other philosophers. It is ascribed to Democritus, and is said to have been by him derived from Leucippus. But this doctrine is found also, we are told[4], among the speculations of another intellectual and acute race, the Hindoos. According to some of their philosophical writers, the ultimate elements of matter are atoms, of which it is proved by certain reasonings, that they are each one-sixth of one of the motes that float in the sunbeam.

This early prevalence of controversies of the widest and deepest kind, which even in our day remain undecided, has in it nothing which need surprize us; or, at least, it has in it nothing which is not in conformity

[4] By Mr. Colebrook. *Asiatic Res.* 1824.

with the general course of the history of philosophy. As soon as any ideas are clearly possessed by the human mind, its activity and acuteness in reasoning upon them are such, that the fundamental antitheses and ultimate difficulties which belong to them are soon brought into view. The Greek and Indian philosophers had mastered completely the Idea of Space, and possessed the Idea of Substance in tolerable distinctness. They were, therefore, quite ready, with their lively and subtle minds, to discuss the question of the finite and infinite divisibility of matter, so far as it involved only the ideas of space and of substance, and this accordingly they did with great ingenuity and perseverance.

But the ideas of Space and of Substance are far from being sufficient to enable men to form a complete general view of the constitution of matter. We must add to these ideas, that of mechanical Fôrce with its antagonist Resistance, and that of the Affinity of one kind of matter for another. Now the former of these ideas the ancients possessed in a very obscure and confused manner; and of the latter they had no apprehension whatever. They made vague assumptions respecting the impact and pressure of atoms on each other; but of their mutual attraction and repulsion they never had any conception, except of the most dim and wavering kind; and of an affinity different from mere local union they did not even dream. Their speculations concerning atoms, therefore, can have no value for us, except as a part of the history of science. If their doctrines appear to us to approach near to the conclusions of our modern philosophy, it must be because our modern philosophy is that philosophy which has not fully profited by the additional light which the experiments and meditations of later times have thrown upon the constitution of matter.

6. *Bacon.*—Still, when modern philosophers look upon the Atomic Theory of the ancients in a general point of view merely, without considering the special conditions which such a theory must fulfil, in order to represent the discoveries of modern times, they are

disposed to regard it with admiration. Accordingly we find Francis Bacon strongly expressing such a feeling. The Atomic Theory is selected and dwelt upon by him as the chain which connects the best parts of the physical philosophy of the ancient and the modern world. Among his works is a remarkable dissertation *On the Philosophy of Democritus, Parmenides, and Telesius:* the last mentioned of whom was one of the revivers of physical science in modern times. In this work he speaks of the atomic doctrine of Democritus as a favourable example of the exertions of the undisciplined intellect. 'Hæc ipsa placita, quamvis paulo emendatiora, talia sunt qualia esse possunt illa quæ ab intellectu sibi permisso, nec continenter et gradatim sublevato, profecta videntur.'—'These doctrines, thus [in an ancient fable] presented in a better form, are such glimpses of truth as can be obtained by the intellect left to its own natural impulses, and not ascending by successive and connected steps,' [as the Baconian philosophy directs]. 'Accordingly,' he adds, 'the doctrine of Atoms, from its going a step beyond the period in which it was advanced, was ridiculed by the vulgar, and severely handled in the disputations of the learned, notwithstanding the profound acquaintance with physical science by which its author was allowed to be distinguished, and from which he acquired the character of a magician.'

'However,' he continues, 'neither the hostility of Aristotle, with all his skill and vigour in disputation, (though, like the Ottoman sultans, he laboured to destroy all his brother philosophers that he might rest undisputed master of the throne of science,) nor the majestic and lofty authority of Plato, could effect the subversion of the doctrine of Democritus. And while the opinions of Plato and Aristotle were rehearsed with loud declamation and professorial pomp in the schools, this of Democritus was always held in high honour by those of a deeper wisdom, who followed in silence a severer path of contemplation. In the days of Roman speculation it kept its ground and its favour; Cicero everywhere speaks of its author with the great-

est praise; and Juvenal, who, like poets in general, probably expressed the prevailing judgment of his time, proclaims his merit as a noble exception to the general stupidity of his countrymen.

. . . . Cujus prudentia monstrat
Magnos posse viros et magna exempla daturos
Vervecum in patriâ crassoque sub aere nasci.

'The destruction of this philosophy was not effected by Aristotle and Plato, but by Genseric and Attila, and their barbarians. For then, when human knowledge had suffered shipwreck, those fragments of the Aristotelian and Platonic philosophy floated on the surface like things of some lighter and emptier sort, and so were preserved; while more solid matters went to the bottom, and were almost lost in oblivion.'

7. *Modern Prevalence of the Atomic Doctrine.*—It is our business here to consider the doctrine of Atoms only in its bearing upon existing physical sciences, and I must therefore abstain from tracing the various manifestations of it in the schemes of hypothetical cosmologists;—its place among the *vortices* of Descartes, its exhibition in the *monads* of Leibnitz. I will, however, quote a passage from Newton to show the hold it had upon his mind.

At the close of his *Opticks* he says, 'All these things being considered, it seems probable to me that God, in the beginning, formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties, and in such proportions to space, as most conduced to the end for which He formed them; and that the primitive particles, being solids, are incomparably harder than any porous bodies compounded of them, even so very hard as never to wear or break in pieces; no ordinary power being able to divide what God had made one in the first creation. While the particles continue entire, they may compose bodies of one and the same nature and texture in all ages: but should they wear away or break in pieces, the nature of things depending on them would be changed. Water and earth composed

of old worn particles and fragments of particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning. And therefore that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles; compounded bodies being apt to break, not in the midst of solid particles, but where those particles are laid together and only touch in a few points.'

We shall hereafter see how extensively the atomic doctrine has prevailed among still more recent philosophers. Not only have the chemists assumed it as the fittest form for exhibiting the principles of multiple proportions; but the physical mathematicians, as Laplace and Poisson, have made it the basis of their theories of heat, electricity, capillary action; and the crystallographers have been supposed to have established both the existence and the arrangement of such ultimate molecules.

In the way in which it has been employed by such writers, the hypothesis of ultimate particles has been of great use, and is undoubtedly permissible. But when we would assert this theory, not as a convenient hypothesis for the expression or calculation of the laws of nature, but as a philosophical truth respecting the constitution of the universe, we find ourselves checked by difficulties of reasoning which we cannot overcome, as well as by conflicting phenomena which we cannot reconcile. I will attempt to state briefly the opposing arguments on this question.

8. *Arguments for and against Atoms.*—The leading arguments on the two sides of the question, in their most general form, may be stated as follows:

For the Atomic Doctrine.—The appearances which nature presents are compounded of many parts, but if we go on resolving the larger parts into smaller, and so on successively, we must at last come to something simple. For that which is compound can be so no otherwise than by composition of what is simple; and if we suppose all composition to be removed, which

hypothetically we may do, there can remain nothing but a number of simple substances, capable of composition, but themselves not compounded. That is, matter being dissolved, resolves itself into atoms.

Against the Atomic Doctrine.—Space is divisible without limit, as may be proved by Geometry; and matter occupies space, therefore matter is divisible without limit, and no portion of matter is *indivisible*, or an *atom*.

And to the argument on the other side just stated, it is replied that we cannot even hypothetically divest a body of composition, if by composition we mean the relation of point to point in space. However small be a particle, it is compounded of parts having relation in space.

The Atomists urge again, that if matter be infinitely divisible, a finite body consists of an infinite number of parts, which is a contradiction. To this it is replied, that the finite body consists of an infinite number of parts in the same sense in which the parts are infinitely small, which is no contradiction.

But the opponents of the Atomists not only rebut, but retort this argument drawn from the notion of infinity. Your atoms, they say, are indivisible by any finite force; therefore they are infinitely hard; and thus your finite particles possess infinite properties. To this the Atomists are wont to reply, that they do not mean the hardness of their particles to be infinite, but only so great as to resist all usual natural forces. But here it is plain that their position becomes untenable; for, in the first place, their assumption of this precise degree of hardness in the particles is altogether gratuitous; and in the next place, if it were granted, such particles are not atoms, since in the next moment the forces of nature may be augmented so as to divide the particle, though hitherto undivided.

Such are the arguments for and against the Atomic Theory in its original form. But when these atoms are conceived, as they have been by Newton, and commonly by his followers, to be solid, hard particles exerting attractive and repulsive forces, a new set of

arguments come into play. Of these, the principal one may be thus stated: According to the Atomic Theory thus modified, the properties of bodies depend upon the attractions and repulsions of the particles. Therefore, among other properties of bodies, their hardness depends upon such forces. But if the hardness *of the bodies* depends upon the forces, the repulsion, for instance, of the particles, upon what does the hardness *of the particles* depend? what progress do we make in explaining the properties of bodies, when we assume the same properties in our explanation? and to what purpose do we assume that the particles *are* hard?

9. *Transition to Boscovich's Theory.*—To this difficulty it does not appear easy to offer any reply. But if the hardness and solidity of the particles be given up as an incongruous and untenable appendage to the Newtonian view of the Atomic Theory, we are led to the theory of Boscovich, according to which matter consists not of solid particles, but of mere mathematical centers of force. According to this theory, each body is composed of a number of geometrical points from which emanate forces, following certain mathematical laws in virtue of which the forces become, at certain small distances attractive, at certain other distances repulsive, and at greater distances attractive again. From these forces of the points arise the cohesion of the parts of the same body, the resistance which it exerts against the pressure of another body, and finally the attraction of gravitation which it exerts upon bodies at a distance.

This theory is at least a homogeneous and consistent theory, and it is probable that it may be used as an instrument for investigating and expressing true laws of nature; although, as we have already said, the attempt to identify the forces by which the particles of bodies are bound together with mechanical attraction, appears to be a confusion of two separate ideas[5].

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[5] 'Boscovich's Theory,' that all bodies may be considered as consisting of a mere collection of centers of forces, may be so conceived as possibly to involve an explanation of all the powers which their parts exert,

10. *Use of the Molecular Hypothesis.*—In this form, representing matter as a collection of molecules or centers of force, the Atomic Theory has been abundantly employed in modern times as an hypothesis on which calculations respecting the elementary forces of bodies might be conducted. When thus employed it is to be considered as expressing the principle that the properties of bodies depend upon forces emanating from immovable points of their mass. This view of the way in which the properties of bodies are to be treated by the mechanical philosopher was introduced by Newton, and was a natural sequel to the success which he had obtained by reasoning concerning central forces on a large scale. I have already quoted his Preface to the *Principia*, in which he says, 'Many things induce me to believe that the rest of the phenomena of nature, as well as those of astronomy, may depend upon certain forces by which the particles of bodies, in virtue of causes not yet known, are urged towards each other and cohere in regular figures, or are mutually repelled and recede; and philosophers, knowing nothing of these forces, have hitherto failed in their examination of nature.' Since the time of Newton, this line of speculation has been followed with great assiduity, and by some mathematicians with great success. In particular Laplace has shown that the hypothesis may, in many instances, be made a much closer representation of nature, if we suppose the forces exerted by the particles to decrease so rapidly with the increasing distance from them, that

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(such powers, namely, as those which produce optical, thermotical and chemical phenomena;) but this theory cannot supply an explanation of the mechanical properties of a body as a whole, especially of its *inertia*. A collection of mere centers of force can have no inertia. If two bodies are considered as two collections of centers of force, the one attracting the other, there is in this view nothing to limit or determine the velocity with which the one body will approach the other. A world composed of such bodies is not a *material* world: for matter (as we have already seen in book iii. chapter v.) implies not only force, but something which resists the action of force.

the force is finite only at distances imperceptible to our senses, and vanishes at all remoter points. He has taught the method of expressing and calculating such forces, and he and other mathematicians of his school have applied this method to many of the most important questions of physics; as capillary action, the elasticity of solids, the conduction and radiation of heat. The explanation of many apparently unconnected and curious observed facts by these mathematical theories gives a strong assurance that its essential principles are true. But it must be observed that the actual constitution of bodies as composed of distinct and separate particles is by no means proved by these coincidences. The assumption, in the reasoning, of certain centers of force acting at a distance, is to be considered as nothing more than a method of reducing to calculation that view of the constitution of bodies which supposes that they exert force at *every* point. It is a mathematical artifice of the same kind as the hypothetical division of a body into infinitesimal parts, in order to find its center of gravity; and no more implies a physical reality than that hypothesis does.

11. *Poisson's Inference.*—When, therefore, M. Poisson, in his views of Capillary Action, treats this hypothetical distribution of centers of force as if it were a physical fact, and blames Laplace for not taking account of their different distribution at the surface of the fluid and below it[6], he appears to push the claims of the molecular hypothesis too far. The only ground for the assumption of separate centers, is that we can thus explain the action of the whole mass. The intervals between the centers nowhere enter into this explanation: and therefore we can have no reason for assuming these intervals different in one part of the fluid and in the other. M. Poisson asserts that the density of the fluid diminishes when we approach very near the surface; but he allows that this diminution is not detected by experiment, and that the formulæ on

[6] Poisson, *Théorie de l'Action Capillaire.*

his supposition, so far as the results go, are identical with those of Laplace. It is clear, then, that his doctrine consists merely in the assertion of the necessary truth of a part of the hypothesis which cannot be put to the test of experiment. It is true, that so long as we have before us the hypothesis of separate centers, the particles very near the surface are not in a condition symmetrical with that of the others: but it is also true that this hypothesis is only a step of calculation. There results, at one period of the process of deduction, a stratum of smaller density at the surface of the fluid; but at a succeeding point of the reasoning the thickness of this stratum vanishes; it has no physical existence.

Thus the *molecular* hypothesis, as used in such cases, does not differ from the doctrine of forces acting at *every point* of the mass; and this principle, which is common to both the opposite views, is the true part of each.

12. *Wollaston's Argument.*—An attempt has been made in another case, but depending on nearly the same arguments, to bring the doctrine of ultimate atoms to the test of observation. In the case of the air, we know that there *is* a diminution of density in approaching the upper surface of the atmosphere, if it have a surface: but it is held by some that except we allow the doctrine of ultimate molecules, it will not be bounded by any surface, but will extend to an infinite distance. This is the reasoning of Wollaston[7]. 'If air consists of any ultimate particles no longer divisible, then must the expansion of the medium composed of them cease at that distance where the force of gravity downwards is equal to the resistance arising from the repulsive force of the medium.' But if there be no such ultimate particles, every stratum will require a stratum beyond it to prevent by its weight a further expansion, and thus the atmosphere

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[7] *Phil. Trans.* 1822, p. 89.

must extend to an infinite distance. And Wollaston conceived that he could learn from observation whether the atmosphere was thus diffused through all space; for if so, it must, he argued, be accumulated about the larger bodies of the system, as Jupiter and the Sun, by the law of universal gravitation; and the existence of an atmosphere about these bodies, might, he remarked, be detected by its effects in producing refraction. His result is, that 'all the phenomena accord entirely with the supposition that the earth's atmosphere is of finite extent, limited by the weight of ultimate atoms of definite magnitude, no longer divisible by repulsion of their parts.'

A very little reflection will show us that such a line of reasoning cannot lead to any result. For we know nothing of the law which connects the density with the compressing force, in air so extremely rare as we must suppose it to be near the boundary of the atmosphere. Now there are possible laws of dependence of the density upon the compressing force such that the atmosphere would terminate in virtue of the law without any assumption of atoms. This may be proved by mathematical reasoning. If we suppose the density of air to be as the square root of the compressing force, it will follow that at the very limits of the atmosphere, the strata of equal thickness may observe in their densities such a law of proportion as is expressed by the numbers 7, 5, 3, 1[8].

If it be asked how, on this hypothesis, the density of the highest stratum can be as 1, since there is no-

[8] For the compressing force on each being as the whole weight beyond it, it will be for the four highest strata, 16, 9, 4 and 1, of which the square roots are as 4, 3, 2, 1, or, as 8, 6, 4, 2; and though these numbers are not exactly as the densities 7, 5, 3, 1, those who are a little acquainted with mathematical reasoning, will see that the difference arises from taking so small a number of strata. If we were to make the strata indefinitely thin, as to avoid error we ought to do, the coincidence would be exact; and thus, according to this law, the series of strata terminates as we ascend, without any consideration of atoms.

thing to compress it, we answer that the upper part of the highest stratum compresses the lower, and that the density diminishes continually to the surface, so that the need of compression and the compressing weight vanish together.

The fallacy of concluding that because the height of the atmosphere is finite, the weight of the highest stratum must be finite, is just the same as the fallacy of those who conclude that when we project a body vertically upwards, because it occupies only a finite time in ascending to the highest point, the velocity at the last instant of the ascent must be finite. For it might be said, if the last velocity of ascent be not finite, how can the body describe the last particle of space in a finite time? and the answer is, that there is no last finite particle of space, and therefore no last finite velocity.

13. *Permanence of Properties of Bodies.*—We have already seen that, in explaining the properties of matter as we find them in nature, the assumption of solid, hard, indestructible particles is of no use or value. But we may remark, before quitting the subject, that Newton appears to have had another reason for assuming such particles, and one well worthy of notice. He wished to express, by means of this hypothesis, the doctrine that the laws of nature do not alter with the course of time. This we have already seen in the quotation from Newton. 'The ultimate particles of matter are indestructible, unalterable, impenetrable; for if they could break or wear, the structure of material bodies now would be different from that which it was when the particles were new.' No philosopher will deny the truth which is thus conveyed by the assertion of atoms; but it is obviously equally easy for a person who rejects the atomic view, to state this truth by saying that the forces which matter exerts do not vary with time, but however modified by the new modifications of its form, are always unimpaired in quantity, and capable of being restored to their former mode of action.

We now proceed to speculations in which the fundamental conceptions may, perhaps, be expressed, at least in some cases, by means of the arrangement of atoms; but in which the philosophy of the subject appears to require a reference to a new Fundamental Idea.

BOOK VII.

THE

PHILOSOPHY

OF

MORPHOLOGY,

INCLUDING

CRYSTALLOGRAPHY.

CRYSTALLIZATION exhibits to us the effects of the natural arrangement of the ultimate particles of various compound bodies; but we are scarcely yet sufficiently acquainted with chemical synthesis and analysis to understand the rationale of this process. The rhomboidal form may arise from the proper position of 4, 6, 8 or 9 globular particles, the cubic form from 8 particles, the triangular form from 3, 6 or 10 particles, the hexahedral prism from 7 particles, &c. Perhaps, in due time we may be enabled to ascertain the number and order of elementary particles, constituting any given compound element, and from that determine the figure which it will prefer on crystallization, and *vice versâ.*

JOHN DALTON, *Chemical Philosophy* (1808), p. 210.

BOOK VII.

THE PHILOSOPHY OF MORPHOLOGY, INCLUDING CRYSTALLOGRAPHY.

CHAPTER I.

EXPLICATION OF THE IDEA OF SYMMETRY.

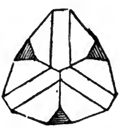
1. WE have seen in the History of the Sciences, that the principle which I have there termed[1] the Principle of Developed and Metamorphosed Symmetry, has been extensively applied in botany and physiology, and has given rise to a province of science termed Morphology. In order to understand clearly this principle, it is necessary to obtain a clear idea of the Symmetry of which we thus speak. But this Idea of Symmetry is applicable in the inorganic, as well as in the organic kingdoms of nature; it is presented to our eyes in the forms of minerals, as well as of flowers and animals; we must, therefore, take it under our consideration here, in order that we may complete our view of Mineralogy, which, as I have repeatedly said, is an essential part of Chemical science. I shall accordingly endeavour to unfold the Idea of Symmetry with which we here have to do.

It will of course be understood that by the term *Symmetry* I here intend, not that more indefinite attribute of form which belongs to the domain of the fine arts, as when we speak of the 'symmetry' of an edifice

[1] *Hist. Ind. Sc.* b. xvii. c. vi.

or of a sculptured figure, but a certain definite relation or property, no less rigorous and precise than other relations of number and position, which is thus one of the sure guides of the scientific faculty, and one of the bases of our exact science.

2. In order to explain what Symmetry is in this sense, let the reader recollect that the bodies of animals consist of *two* equal and similar sets of members, the right and the left side;—that some flowers consist of three or of five equal sets of organs, similarly and regularly disposed, as the iris has *three* straight petals, and three reflexed ones, alternately disposed, the rose has *five* equal and similar sepals of the calyx, and alternate with these, as many petals of the corolla. This orderly and exactly similar distribution of two, or three, or five, or any other number of parts, is Symmetry; and according to its various modifications, the forms thus determined are said to be *symmetrical* with various numbers of members. The classification of these different kinds of symmetry has been most attended to in Crystallography, in which science it is the highest and most general principle by which the classes of forms are governed. Without entering far into the technicalities of the subject, we may point out some of the features of such classes.



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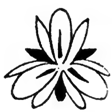
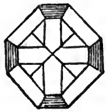


The first of the figures (1) in the margin may represent the summit of a crystal as it appears to an eye looking directly down upon it; the center of the figure represents the summit of a pyramid, and the spaces of various forms which diverge from this point represent sloping sides of the pyramid. Now it will be observed that the figure consists of three portions exactly similar to one another, and that each part or member is repeated in each of these portions. The faces, or pairs of faces, are repeated in *threes*, with exactly similar forms and angles. This figure is said to be *three-membered*, or to have *triangular* symmetry. The same kind of sym-

metry may exist in a flower, as presented in the accompanying figure, and does, in fact, occur in a large class of flowers, as for example, all the lily tribe. The next pair of figures (2) have four equal and similar portions, and have their members or pairs of members four times repeated. Such figures are termed *four-membered*, and are said to have *square* or *tetragonal* symmetry. The *pentagonal* symmetry, formed by *five* similar *members*, is represented in the next figures (3). It occurs abundantly in the vegetable world, but never among crystals; for the pentagonal figures which crystals sometimes assume, are never exactly regular. But there is still another kind of symmetry (4) in which the opposite ends are exactly similar to each other and also the opposite sides; this is *oblong*, or *two-and-two-membered* symmetry. And finally, we have the case of *simple* symmetry (5) in which the two sides of the object are exactly alike (in opposite positions) without any further repetition.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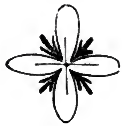

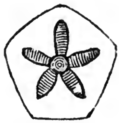
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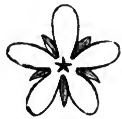

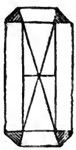
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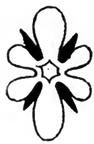

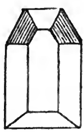
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3. These different kinds of symmetry occur in various ways in the animal, vegetable, and mineral kingdom. Vertebrate animals have a right and a

left side exactly alike, and thus possess *simple* symmetry. The same kind of symmetry (simple symmetry) occurs very largely in the forms of vegetables, as in most leaves, in *papilionaceous*, *personate*, and *labiate* flowers. Among minerals, crystals which possess this symmetry are called *oblique-prismatic*, and are of very frequent occurrence. The *oblong*, or *two-and-two-membered* symmetry belongs to *right-prismatic* crystals; and may be seen in *cruciferous* flowers, for though these are cross-shaped, the cross has two longer and two shorter arms, or pairs of arms. The *square* or *tetragonal* symmetry occurs in crystals abundantly; to the vegetable world it appears to be less congenial; for though there are flowers with four exactly similar and regularly-disposed petals, as the herb Paris (*Paris quadrifolia*), these flowers appear, from various circumstances, to be deviations from the usual type of vegetable forms. The *trigonal*, or *three-membered* symmetry is found abundantly both in plants and in crystals, while the *pentagonal* symmetry, on the other hand, though by far the most common among flowers, nowhere occurs in minerals, and does not appear to be a possible form of crystals. This pentagonal form further occurs in the animal kingdom, which the oblong, triangular, and square forms do not. Many of Cuvier's *radiate* animals appear in this pentagonal form, as *echini* and *pentacrinites*, which latter have hence their name.

4. The regular, or as they may be called, the *normal* types of the vegetable world appear to be the forms which possess triangular and pentagonal symmetry; from these the others may be conceived to be derived, by transformations resulting from the expansion of one or more parts. Thus it is manifest that if in a three-membered or five-membered flower, one of the petals be expanded more than the other, it is immediately reduced from pentagonal or trigonal, to simple symmetry. And the oblong or two-and-two-membered symmetry of the flowers of cruciferous plants, (in which the stamens are four large and two small ones, arranged in regular opposition,) is held by botanists to result

from a normal form with ten stamens; Meinecke explaining this by adhesion, and Sprengel by the metamorphosis of the stamens into petals[2].

It is easy to see that these various kinds of symmetry include relations both of form and of number, but more especially of the latter kind; and as this symmetry is often an important character in various classes of natural objects, such classes have often curious numerical properties. One of the most remarkable and extensive of these is the distinction which prevails between monocotyledonous and dicotyledonous plants; the number *three* being the ground of the symmetry of the former, and the number *five*, of the latter. Thus liliaceous and bulbous plants, and the like, have flowers of three or six petals, and the other organs follow the same numbers: while the vast majority of plants are pentandrous, and with their five stamens have also their other parts in fives. This great numerical distinction corresponding to a leading difference of physiological structure cannot but be considered as a highly curious fact in phytology. Such properties of numbers, thus connected in an incomprehensible manner with fundamental and extensive laws of nature, give to numbers an appearance of mysterious importance and efficacy. We learn from history how strongly the study of such properties, as they are exhibited by the phenomena of the heavens, took possession of the mind of Kepler; perhaps it was this which, at an earlier period, contributed in no small degree to the numerical mysticism of the Pythagoreans in antiquity, and of the Arabians and others in the middle ages. In crystallography, numbers are the primary characters in which the properties of substances are expressed;—they appear, first, in that classification of forms which depends on the degree of symmetry, that is, upon the number of correspondencies; and next, in the laws of derivation, which, for the most part, appear to be common in their occurrence in proportion to the numerical simplicity of their expression. But the manifestation

[2] Sprengel, *Gesch. d. Bot.* ii. 304.

of a governing numerical relation in the organic world strikes us as more unexpected; and the selection of the number *five* as the index of the symmetry of dicotyledonous plants and radiated animals, (a number which is nowhere symmetrically produced in inorganic bodies,) makes this a new and remarkable illustration of the constancy of numerical relations. We may observe, however, that the moment one of these radiate animals has one of its five members expanded, or in any way peculiarly modified, (as happens among the echini), it is reduced to the common type of animals simply symmetrical, with a right and left side.

5. It is not necessary to attempt to enumerate all the kinds of Symmetry, since our object is only to explain what Symmetry is, and for this purpose enough has probably been said already. It will be seen, as soon as the notion of Symmetry in general is well apprehended, that it is or includes a peculiar Fundamental Idea, not capable of being resolved into any of the ideas hitherto examined. It may be said, perhaps, that the Idea of Symmetry is a modification or derivative of our ideas of space and number;—that a symmetrical shape is one which consists of parts exactly similar, repeated a certain number of times, and placed so as to correspond with each other. But on further reflection it will be seen that this repetition and correspondence of parts in symmetrical figures are something peculiar; for it is not *any* repetition or any correspondence of parts to which we should give the name of symmetry, in the manner in which we are now using the term. Symmetrical arrangements may, no doubt, be concerned with space and position, time and number; but there appears to be implied in them a Fundamental Idea of regularity, of completeness, of complex simplicity, which is not a mere modification of other ideas.

6. It is, however, not necessary, in this and in similar cases, to determine whether the idea which we have before us be a peculiar and independent Fundamental Idea or a modification of other ideas, provided we clearly perceive the evidence of those Axioms by

means of which the Idea is applied in scientific reasonings. Now in the application of the Idea of Symmetry to crystallography, phytology and zoology, we must have this idea embodied in some principle which asserts more than a mere geometrical or numerical accordance of members. We must have it involved in some vital or productive action, in order that it may connect and explain the facts of the organic world. Nor is it difficult to enunciate such a principle. We may state it in this manner. *All the symmetrical members of a natural product are, under like circumstances, alike affected by the natural formative power.* The parts which we have termed *symmetrical*, resemble each other, not only in their form and position, but also in the manner in which they are produced and modified by natural causes. And this principle we assume to be necessarily true, however unknown and inconceivable may be the causes which determine the phenomena. Thus it has not yet been found possible to discover or represent to ourselves, in any intelligible manner, the forces by which the various faces of a crystal are consequent upon its primary form: for the hypothesis of their being built up of integrant molecules, as Haüy held, cannot be made satisfactory. But though the mechanism of crystals is still obscure, there is no doubt as to the principle which regulates their modifications. The whole of crystallography rests upon this principle, that if one of the primary planes or axes be modified in any manner, all the symmetrical planes and axes must be modified in the same manner. And though accidental mechanical or other causes may interfere with the actual exhibition of such faces, we do not the less assume their crystallographical reality, as inevitably implied in the law of symmetry of the crystal[3]. And we apply similar considerations to organized beings. We assume that in a regular flower, each of the similar

[3] Some crystalline forms, instead of being *holohedral* (provided with their whole number of faces), are *hemihedral* (provided with only half their number of faces). But in these hemihedral forms the half of the faces are still *symmetrically* suppressed.

members has the same organization and similar powers of developement; and hence if among these similar parts some are much less developed than others, we consider them as *abortive;* and if we wish to remove doubts as to what are symmetrical members in such a case, we make the inquiry by tracing the anatomy of these members, or by following them in their earlier states of developement, or in cases where their capabilities are magnified by monstrosity or otherwise. The power of developement may be modified by external causes, and thus we may pass from one kind of symmetry to another; as we have already remarked. Thus a regular flower with pentagonal symmetry, growing on a lateral branch, has one petal nearest to the axis of the plant: if this petal be more or less expanded than the others, the pentagonal symmetry is interfered with, and the flower may change to a symmetry of another kind. But it is easy to see that all such conceptions of *expansion*, *abortion*, and any other kind of *metamorphosis*, go upon the supposition of identical faculties and tendencies in each similar member, in so far as such tendencies have any relation to the symmetry. And thus the principle we have stated above is the basis of that which, in the History, we termed the Principle of Developed and Metamorphosed Symmetry.

We shall not at present pursue the other applications of this Idea of Symmetry, but we shall consider some of the results of its introduction into Crystallography.

CHAPTER II.

Application of the Idea of Symmetry to Crystals.

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1. MINERALS and other bodies of definite chemical composition often exhibit that marked regularity of form and structure which we designate by terming them *Crystals;* and in such crystals, when we duly study them, we perceive the various kinds of symmetry of which we have spoken in the previous chapter. And the different kinds of symmetry which we have there described are now usually distinguished from each other, by writers on crystallography. Indeed it is mainly to such writers that we are indebted for a sound and consistent classification of the kinds and degrees of symmetry of which forms are capable. But this classification was by no means invented as soon as mineralogists applied themselves to the study of crystals. These first attempts to arrange crystalline forms were very imperfect; those, for example, of Linnæus, Werner, Romé de Lisle, and Haüy. The essays of these writers implied a classification at once defective and superfluous. They reduced all crystals to one or other of certain *fundamental forms;* and this procedure might have been a perfectly good method of dividing crystalline forms into classes, if the fundamental forms had been selected so as to exemplify the different kinds of symmetry. But this was not the case. Haüy's fundamental or 'primitive' forms, were, for instance, the following: the *parallelepiped,* the *octahedron,* the *tetrahedron,* the *regular hexagonal prism,* the *rhombic dodecahedron,* and the *double hexagonal pyramid.* Of these, the *octahedron,* the *tetrahedron,* the *rhombic dodecahedron,* all belong to the

same kind of symmetry (the TESSULAR systems); also the *hexagonal prism* and the *hexagonal pyramid* both belong to the RHOMBIC system; while the *parallelepiped* is so employed as to include all kinds of symmetry.

It is, however, to be recollected that Haüy, in his selection of primitive forms, not only had an eye to the external form of the crystal and to its degree and kind of regularity, but also made his classification with an especial reference to the *cleavage* of the mineral, which he considered as a primary element in crystalline analysis. There can be no doubt that the cleavage of a crystal is one of its most important characters: it is a relation of form belonging to the interior, which is to be attended to no less than the form of the exterior. But still, the cleavage is to be regarded only as determining the degree of geometrical symmetry of the body, and not as defining a special geometrical figure to which the body *must* be referred. To have looked upon it in the latter light, was a mistake of the earlier crystallographic speculators, on which we shall shortly have to remark.

2. I have said that the reference of crystals to Primitive Forms might have been well employed as a mode of expressing a just classification of them. This follows as a consequence from the application of the Principle stated in the last chapter, that *all symmetrical members are alike affected.* Thus we may take an upright triangular prism as the representative of the rhombic system, and if we then suppose one of the upper edges to be cut off, or *truncated*, we must, by the Principle of Symmetry, suppose the other two upper edges to be truncated in precisely the same manner. By this truncation we may obtain the upper part of a rhombohedron; and by truncations of the same kind, symmetrically affecting all the analogous parts of the figure, we may obtain any other form possessing three-membered symmetry. And the same is true of any of the other kinds of symmetry, provided we make a proper selection of a fundamental form. And this was really the method employed by Demeste, Werner, and Romé de Lisle. They

assumed a Primitive Form, and then conceived other forms, such as they found in nature, to be derived from the Primitive Form by truncation of the edges, acumination of the corners, and the like processes. This mode of conception was a perfectly just and legitimate expression of the general Idea of Symmetry.

3. The true view of the degrees of symmetry was, as I have already said, impeded by the attempts which Haüy and others made to arrive at primitive forms by the light which cleavage was supposed to throw upon the structure of minerals. At last, however, in Germany, as I have narrated in the History of Mineralogy[1], Weiss and Mohs introduced a classification of forms implying a more philosophical principle, dividing the forms into *Systems;* which, employing the terms of the latter writer, we shall call the *tessular*, the *pyramidal* or *square pyramidal*, the *prismatic* or *oblong*, and the *rhombohedral* systems.

Of these forms, the three latter may be at once referred to those kinds of symmetry of which we have spoken in the last chapter. The *rhombohedral* system has *triangular* symmetry, or is three-membered: the *pyramidal* has *square* symmetry, or is four-membered: the *prismatic* has *oblong* symmetry, and is two-and-two-membered. But the kinds of symmetry which were spoken of in the former chapter, do not exhaust the idea when applied to minerals. For the symmetry which was there explained was such only as can be exhibited on a surface, whereas the forms of crystals are solid. Not only have the right and left parts of the upper surface of a crystal relations to each other; but the upper surface and the lateral faces of the crystal have also their relations; they may be different, or they may be alike. If we take a cube, and hold it so that four of its faces are vertical, not only are all these four sides exactly similar, so as to give square symmetry; but also we may turn the cube, so that any one of these four sides shall become the top, and still the four sides which are thus made vertical, though

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1 *Hist. Ind. Sc.* b. xv. c. iv.

not the same which were vertical before, are still perfectly symmetrical. Thus this cubical figure possesses more than square symmetry. It possesses square symmetry in a vertical as well as in a horizontal sense. It possesses a symmetry which has the same relation to a *cube* which four-membered symmetry has to a *square*. And this kind of symmetry is termed the *cubical* or *tessular* symmetry. All the other kinds of symmetry have reference to an axis, about which the corresponding parts are disposed; but in tessular symmetry the horizontal and vertical axes are also symmetrical, or interchangeable; and thus the figure may be said to have no axis at all.

4. It has already been repeatedly stated that, by the very idea of symmetry, all the incidents of form must affect alike all the corresponding parts. Now in crystals we have, among these incidents, not only external figure, but *cleavage*, which may be considered as internal figure. Cleavage, then, must conform to the degree of symmetry of the figure. Accordingly cleavage, no less than form, is to be attended to in determining to what system a mineral belongs. If a crystal were to occur as a square prism or pyramid, it would not on that account necessarily belong to the square pyramidal system. If it were found that it was cleavable parallel to one side of the prism, but not in the transverse direction, it has only oblong symmetry; and the equality of the sides which makes it square is only accidental.

Thus no cleavage is admissible in any system of crystallization which does not agree with the degree of symmetry of the system. On the other hand, *any* cleavage which *is* consistent with the symmetry of the system, is (hypothetically at least) allowable. Thus in the oblong prismatic system we may have a cleavage parallel to one side only of the prism; or parallel to both, but of different distinctness; or parallel to the two diagonals of the prism but of the same distinctness; or we may have both these cleavages together. In the rhombohedral system, the cleavage may be parallel to the sides of the rhombohedron, as in Calc

Spar: or, in the same system, the cleavage, instead of being thus oblique to the axis, may be along the axis in those directions which make equal angles with each other: this cleavage easily gives either a triangular or a hexagonal prism. Again, in the tessular system, the cleavage may be parallel to the surface of the cube, which is thus readily separable into other cubes, as in Galena; or the cleavage may be such as to cut off the solid angle of the cube, and since there are eight of these, such cleavage gives us an octahedron, which, however, may be reduced to a tetrahedron, by rejecting all parallel faces, as being mere repetitions of the same cleavage; this is the case with Fluor Spar: or the cube of the tessular system may be cleavable in planes which truncate all the edges of the cube; and as these are twelve, we thus obtain the dodecahedron with rhombic faces: this occurs in Zinc Blende. And thus we see the origin of Haüy's various primitive forms, the tetrahedron, octahedron, and rhombic dodecahedron, all belonging to the tessular system:—they are, in fact, different cleavage forms of that system.

5. I do not dwell upon other incidents of crystals which have reference to form, nor upon the lustre, smoothness, and striation of the surfaces. To all such incidents the general principle applies, that similar parts are similarly affected; and hence, if any parts are found to be constantly and definitely different from other parts of the same sort, they are not similar parts; and the symmetry is to be interpreted with reference to this difference.

We have now to consider the inferences which have been drawn from these incidents of crystallization, with regard to the intimate structure of bodies.

CHAPTER III.

Speculations founded upon the Symmetry of Crystals.

1. WHEN a crystal, as, for instance, a crystal of Galena, (sulphuret of lead,) is readily divisible into smaller cubes, and these into smaller ones, and so on without limit, it is very natural to represent to ourselves the original cube as really consisting of small cubical elements; and to imagine that it is a philosophical account of the physical structure of such a substance to say that it is made up of cubical molecules. And when the Galena crystal has externally the form of a cube, there is no difficulty in such a conception; for the surface of the crystal is also conceived as made up of the surfaces of its cubical molecules. We conceive the crystal so constituted, as we conceive a wall built of bricks.

But if, as often happens, the Galena crystal be an octahedron, a further consideration is requisite in order to understand its structure, pursuing still the same hypothesis. The mineral is still, as in the other case, readily cleavable into small cubes, having their corners turned to the faces of the octahedron. Therefore these faces can no longer be conceived as made up of the faces of cubical elements of which the whole is constituted. If we suppose a pile of such small cubes to be closely built together, but with decreasing width above, so as to form a pyramid, the face of such a pyramid will no longer be plane; it will consist of a great number of the corners or edges of the small elementary cubes. It would appear at first sight, therefore, that such a face cannot represent the smooth polished surface of a crystal.

But when we come to look more closely, this difficulty disappears. For how large are these elementary cubes? We cannot tell, even supposing they really have any size. But we know that they must be, at any rate, very small; so small as to be inappreciable by our senses, for our senses find no limit to the divisibility of minerals by cleavage. Hence the surface of the pyramid above described would not consist of visible corners or edges, but would be roughened by specks of imperceptible size; or rather, by supposing these specks to become still smaller, the roughness becomes smoothness. And thus we may have a crystal with a smooth surface, made up of small cubes in such a manner that their surfaces are all oblique to the surface of the crystal.

Haüy, struck by some instances in which the supposition of such a structure of crystals appeared to account happily for several of their relations and properties, adopted and propounded it as a general theory. The small elements, of which he supposed crystals to be thus built up, he termed *integrant molecules.* The form of these molecules might or might not be the same as the *primitive form* with which his construction was supposed to begin; but there was, at any rate, a close connexion between these forms, since both of them were founded on the cleavage of the mineral. The tenet that crystals are constituted in the manner which I have been describing, I shall call the *Theory of Integrant Molecules*, and I have now to make some remarks on the grounds of this theory.

2. In the case of which I have spoken, the mineral used as the example, Galena, readily splits into cubes, and cubes are easily placed together so as to fit each other, and fill the space which they occupy. The same is the case in the mineral which suggested to Haüy his theory, namely, Calc Spar. The crystals of this substance are readily divisible into rhombohedrons, a form like a brick with oblique angles; and such bricks can be built together so as to produce crystals of all the immense varieties of form which Calc Spar presents. This kind of masonry is equally possible in many other

minerals; but as we go through the mineral kingdom in our survey, we soon find cases which offer difficulties. Some minerals cleave only in two directions, some in one only; in such cases we cannot by cleavage obtain an integrant molecule of definite form; one of its dimensions, at least, must remain indeterminate and arbitrary. Again, in some instances, we have more than three different planes of cleavage, as in Fluor Spar, where we have four. The solid, bounded by four planes, is a tetrahedron; or if we take four *pairs* of parallel faces, an octahedron. But if we attempt to take either of these forms for our integrant molecule, we are met by this difficulty: that a collection of such forms will not fill space. Perhaps this difficulty will be more readily conceived by the general reader if it be contemplated with reference to plane figures. It will readily be seen that a number of equal squares may be put together so as to fill the space which they occupy; but if we take a number of equal regular octagons, we may easily convince ourselves that no possible arrangement can make them cover a flat space without leaving blank spots between. In like manner octahedrons or tetrahedrons cannot be arranged in solid space so as to fill it. They necessarily leave vacancies. Hence the structure of Fluor Spar, and similar crystals, was a serious obstacle in the way of the theory of integrant molecules. That theory had been adopted in the first instance because portions of the crystal, obtained by cleavage, could be built up into a solid mass; but this ground of the theory failed altogether in such instances as I have described, and hence the theory, even upon the representations of its adherents, had no longer any claim to assent.

The doctrine of Integral Molecules, however, was by no means given up at once, even in such instances. In this and in other subjects, we may observe that a theory, once constructed and carried into detail, has such a hold upon the minds of those who have been in the habit of applying it, that they will attempt to uphold it by introducing suppositions inconsistent with

the original foundations of the theory. Thus those who assert the Atomic Theory, reconcile it with facts by taking the *halves* of *atoms;* and thus the Theory of Integrant Molecules was maintained for Fluor Spar, by representing the elementary octahedrons of which crystals are built up, as touching each other only by the *edges*. The contact of surface with surface amongst integrant molecules had been the first basis of the theory; but this supposition being here inapplicable, was replaced by one which made the theory no longer a representation of the facts (the cleavages), but a mere geometrical construction. Although, however, the inapplicability of the theory to such cases was thus, in some degree, disguised to the disciples of Haüy, it was plain that, in the face of such difficulties, the Theory of Integrant Molecules could not hold its place as a philosophical truth. But it still answered the purpose (a very valuable one, and one to which crystallography is much indebted,) of an instrument for calculating the geometrical relations of the parts of crystals to each other: for the integrant molecules were supposed to be placed layer above layer, each layer as we ascend, *decreasing* by a certain number of molecules and rows of molecules; and the calculation of these *laws of decrement* was, in fact, the best mode then known of determining the positions of the faces. The Theory of Decrements served to express and to determine, in a great number of the most obvious cases, *the laws of phenomena* in crystalline forms, though the Theory of Integrant Molecules could not be maintained as a just view of the structure of crystals.

3. The Theory of Integrant Molecules, however, involved this just and important principle: that a true view of the intimate structure of crystals must include and explain the facts of crystallization, that is, crystalline form and cleavage; and that it must take these into account, according to their degree of *Symmetry.* So far all theories concerning the elements of crystals must agree. And it was soon seen that this was, in reality, all that had been established by the investigations of Haüy and his school. I have already, in the

History, quoted Weiss's reflections on making this step. 'When in 1809,' he says[1], 'I published my Dissertation, I shared the common opinion as to the necessity of the assumption, and the reality of the existence of a primitive form, at least in a sense not very different from the usual sense of the expression.' He then proceeds to relate that he sought a ground for such an opinion, independent of the doctrine of *Atoms*, which he, in common with a great number of philosophers of that time in his own country, was disposed to reject, inclining to believe that the properties of bodies were determined by *Forces* which acted in them, and not by *Molecules* of which they were composed. He adds, that in pursuing this train of thought, he found, 'that out of his Primitive Forms there was gradually unfolded to his hands that which really governs them, and is not affected by their casual fluctuations; namely, the Fundamental Relations of their Dimensions,' or as we now may call them, *Axes of Symmetry*. With reference to these Axes, he found, as he goes on to say, that 'a multiplicity of internal Oppositions, necessarily and mutually interdependent, are developed in the crystalline mass, each Relation having its own Polarity; so that the Crystalline Character is co-extensive with these Polarities.' The character of these polarities, whether manifested in crystalline faces, cleavage, or any other incidents of crystallization, is necessarily displayed in the degree and kind of Symmetry which the crystal possesses: and thus this Symmetry, in all our speculations concerning the structure of crystals, necessarily takes the place of that enumeration of Primitive Forms which were rejected as inconsistent with observed facts, and destitute of sound scientific principle.

I may just notice here what I have stated in the History of Mineralogy[2], that the distinction of systems of crystallization, as introduced by Weiss and Mohs, was strikingly confirmed by Sir David Brewster's discoveries respecting the optical properties of minerals.

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1 *Acad. Berlin.* 1816, p. 307.

2 *Hist. Ind. Sc.* b. xv. c. v.

The splendid phenomena which were produced by passing polarized light through crystals, were found to vary according as the crystals were of the Rhombohedral, Square Pyramidal, Oblong Prismatic, or Tessular System. The Optical Symmetry exactly corresponded with the Geometrical Symmetry. In the two former Systems were crystals *uniaxal* in respect of their optical properties; the oblong prismatic, was *biaxal;* while in the tessular, the want of a predominant axis prevented the phenomena here spoken of from occurring at all. The optical experiments must have led, and would have led, to a classification of crystals into the above systems or something nearly equivalent, even had they not been already so arranged by attention to their forms.

4. While in Germany Weiss and Mohs with their disciples, were gradually rejecting what was superfluous in the previous crystallographical hypotheses, philosophers in England were also trying to represent to themselves the constitution of crystals in a manner which should be free from the obviously arbitrary and untenable fictions of the Haüyian school. These attempts, however, were not crowned with much success. One mode of representing the structure of crystals which suggested itself, was to reject the polyhedral forms which Haüy gave to his integrant molecules, and to conceive the elements of crystals as *spheres*, the properties of the crystal being determined not by the *surfaces*, but by the *position* of the elements. This was done by Wollaston, in the *Philosophical Transactions* for 1813. He applied this view to the tessular system, in which, indeed, the application is not difficult; and he showed that octahedral and tetrahedral figures may be deduced from symmetrical arrangements of equal spherules. But though in doing this, he manifested a perception of the conditions of the problem, he appeared to lose his hold on the real question when he tried to pass on to other systems of crystallization. For he accounted for the rhombohedral system by supposing the spheres changed into *spheroids*. Such a procedure involved him in a gratuitous and useless hypothesis: for to what purpose do we introduce the

arrangement of atoms (instead of their figure,) as a mode of explaining the symmetry of the crystallization, when at the next step we ascribe to the atom, by an arbitrary fiction, a symmetry of figure of the same kind as that which we have to explain? It is just as easy, and as allowable, to assume an elementary rhombohedron, as to assume elementary spheroids, of which the rhombohedrons are constructed.

5. Many hypotheses of the same kind might be adduced, devised both by mineralogists and chemists. But almost all such speculations have been pursued with a most surprising neglect of the principle which obviously is the only sound basis on which they can proceed. The principle is this:—that *All hypotheses concerning the arrangement of the elementary atoms of bodies in space must be constructed with reference to the general facts of crystallization.* The truth and importance of this principle can admit of no doubt. For if we make any hypothesis concerning the mode of connexion of the elementary particles of bodies, this must be done with the view of representing to ourselves the forces which connect them, and the results of these forces as manifested in the properties of the bodies. Now the forces which connect the particles of bodies so as to make them crystalline, are manifestly chemical forces. It is only definite chemical compounds which crystallize; and in crystals the force of cohesion by which the particles are held together cannot in any way be distinguished or separated from the chemical force by which their elements are combined. The elements are understood to be combined, precisely because the result is a definite, apparently homogeneous substance. The properties of the compound bodies depend upon the elements and their mode of combination; for, in fact, these include everything on which they *can* depend. There are no other circumstances than these which can affect the properties of a body. Therefore all those properties which have reference to space, namely, the crystalline properties, cannot depend upon anything else than the arrangement of the elementary molecules in space. These

properties are the facts which any hypothesis of the arrangement of molecules must explain, or at least render conceivable; and all such hypotheses, all constructions of bodies by supposed arrangements of molecules, can have no other philosophical object than to account for facts of this kind. If they do not do this, they are mere arbitrary geometrical fictions, which cannot be in any degree confirmed or authorised by an examination of nature, and are therefore not deserving of any regard.

6. Those philosophers who have endeavoured to represent the mode in which bodies are constructed by the combination of their chemical atoms, have often undertaken to show, not only that the atoms are combined, but also in what positions and configurations they are combined. And it is truly remarkable, as I have already said, that they have done this, almost in every instance, without any consideration of the crystalline character of the resulting combinations; from which alone we receive any light as to the relation of their elements in space. Thus Dr. Dalton, in his *Elements of Chemistry*, in which he gave to the world the Atomic Theory as a representation of the doctrine of definite and multiple proportions, also published a large collection of Diagrams, exhibiting what he conceived to be the configuration of the atoms in a great number of the most common combinations of chemical elements. Now these hypothetical diagrams do not in any way correspond, as to the nature of their symmetry, with the compounds, as we find them displaying their symmetry when they occur crystallized. Carbonate of lime has in reality a triangular symmetry, since it belongs to the rhombohedral system; Dr. Dalton's carbonate of lime would be an oblique rhombic prism or pyramid. Sulphate of baryta is really two-and-two membered; Dr. Dalton's diagram makes it two-and-one membered. Alum is really octahedral or tessular; but according to the diagram it could not be so, since the two ends of the atom are not symmetrical. And the same want of correspondence between the facts and the hypothesis runs through the whole

system. It need not surprise us that the theoretical arrangement of atoms does not explain the facts of *crystallization;* for to produce such an explanation would be a second step in science quite as great as the first, the discovery of the atomic theory in its *chemical* sense. But we may allow ourselves to be surprised that an utter discrepance between all the facts of crystallization and the figures assumed in the theory, did not suggest any doubt as to the soundness of the mode of philosophizing by which this part of the theory was constructed.

7. Some little accordance between the hypothetical arrangements of chemical atoms and the facts of crystallization, does appear to have been arrived at by some of the theorists to whom we here refer, although by no means enough to show a due conviction of the importance of the principle stated above. Thus Wollaston, in the Essay above noticed, after showing that a symmetrical arrangement of equal spherules would give rise to octahedral and other tessular figures, remarks, very properly, that the metals, which are simple bodies, crystallize in such forms. M. Ampère[3] also, in 1814, published a brief account of an hypothesis of a somewhat similar nature, and stated himself to have developed this speculation in a Memoir which has not yet, so far as I am aware, been published. In this notice he conceives bodies to be compounded of *molecules*, which, arranged in a polyhedral form, constitute *particles.* These *representative forms* of the particles depend on chemical laws. Thus the particles of oxygen, of hydrogen, and of azote, are composed each of four molecules. Hence it is collected that the particles of nitrous gas are composed of two molecules of oxygen and two of azote; and similar conclusions are drawn respecting other substances. These conclusions, though expressed by means of the polyhedrons thus introduced, are supported by chemical, rather than by crystallographical comparisons. The author does, indeed, appeal to the crystallization of sal ammo-

[3] *Ann. de Chimie*, tom. xc. p. 43.

niac as an argument[4]; but as *all* the forms which he introduces appear to belong to the *tessular* system of crystallization, there is, in his reasonings, nothing distinctive; and therefore nothing, crystallographically speaking, of any weight on the side of this theory.

8. Any hypothesis which should introduce any principle of chemical order among the actual forms of minerals, would well deserve attention. At first sight, nothing can appear more anomalous than the forms which occur. We have, indeed, one broad fact, which has an encouraging aspect, the tessular forms in which the pure metals crystallize. The highest degree of chemical and of geometrical simplicity coincide: irregularity disappears precisely where it is excluded by the consideration above stated, that the symmetry of chemical composition must determine the symmetry of crystalline form[5].

But if we go on to any other class of crystalline forms, we soon find ourselves lost in our attempts to

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[4] *Ann. de Chimie*, tom. xc. p. 83.

[5] Inasmuch as this law, that the simple metals crystallize in tessular forms, is the most signal example of that connexion between the chemical nature of a body and its crystalline form, I in the former Edition stated it with as much generality as I could find any ground for, and I should have been glad if I could have added confirmation of the law, derived from later observations. But the most recent investigations of crystallographers appear to have afforded exceptions rather than examples of the rule. Arsenic and Tellurium are said to be *rhombohedral*. Antimony, stated by Haüy to be octahedral (and therefore tessular), has been found by more modern observers to be *rhombohedral*. Tin has been obtained by Professor Miller in beautiful crystals belonging to the *pyramidal* system. Professor Nöggerath has observed in Zinc, after cooling from fusion, hexagonal cleavage, rendering it probable that the mineral crystallized in *rhombohedrons* having their axes vertical, like ice. G. Rose conceives it highly probable that Osmium and Iridium are *rhombohedral*. (Poggendorf. Bd. liv.)

But all the more perfect metals are tessular; namely, Gold, Silver, Mercury, Platinum, Iron, Copper; also Bismuth [?] Perhaps the observation in which the crystallization of Zinc is affected by its position is, on that very account, no sufficient evidence of its *free* crystallization. We can hardly conceive a collection of perfectly simple, similar particles to crystallize so as to have one pre-eminent axis, without some extraneous action affecting them.

follow any thread of order. We have indeed many large groups connected by obvious analogies; as the rhombohedral carbonates of lime, magnesia, iron, manganese;—the prismatic carbonates and sulphates of lime, baryta, strontia, lead. But even in these, we cannot form any plausible hypothesis of the arrangement of the elements; and in other cases to which we naturally turn, we can find nothing but confusion. For instance, if we examine the oxides of metals:—those of iron are rhombohedral and tessular; those of copper, tessular; those of tin, of titanium, of manganese, square pyramidal; those of antimony, prismatic; and we have other forms for other substances.

It may be added, that if we take account of the optical properties which, as we have already stated, have constant relations to the crystalline forms, the confusion is still further increased; for the optical dimensions vary in amount, though not in symmetry, where chemistry can trace no difference of composition.

9. We will not quit the subject, however, without noticing the much more promising aspect which it has assumed by the detection of such groups as are referred to in the last article; or in other words, by Mitscherlich's discovery of *Isomorphism.* According to that discovery, there are various elements which may take the place of each other in crystalline bodies, either without any alteration of the crystalline form, or at most with only a slight alteration of its dimensions. Such a group of elements we have in the earths lime and magnesia, the protoxides of iron and manganese: for the carbonates of all these bases occur crystallized in forms of the rhombohedral system, the characteristic angle being nearly the same in all. Now lime and magnesia, by the discoveries of modern chemistry, are really oxides of metals; and therefore all these carbonates have a similar chemical constitution, while they have also a similar crystalline form. Whether or no we can devise any arrangement of molecules by which this connexion of the chemical and the geometrical property can be represented, we cannot help consider-

ing the connexion as an extremely important fact in the constitution of bodies; and such facts are more likely than any other to give us some intelligible view of the relations of the ultimate parts of bodies. The same may be said of all the other isomorphous or plesiomorphous groups[6]. For instance, we have a number of minerals which belong to the same system of crystallization, but in which the chemical composition appears at first sight to be very various: namely, spinelle, pleonaste, gahnite, franklinite, chromic iron oxide, magnetic iron oxide: but Abich has shown that all these may be reduced to a common chemical formula;—they are bioxides of one set of bases, combined with trioxides of another set. Perhaps some mathematician may be able to devise some geometrical arrangement of such a group of elements which may possess the properties of the tessular system. Hypothetical arrangements of atoms, thus expressing both the chemical and the crystalline symmetry which we know to belong to the substance, would be valuable steps in analytical science; and when they had been duly verified, the hypotheses might easily be divested of their atomic character.

Thus, as we have already said, mineralogy, understood in its wider sense, as the counterpart of chemistry, has for one of its main objects to discover those Relations of the Elements of bodies which have reference to Space. In this research, the foundation of all sound speculation is the kind and degree of Symmetry of form which we find in definite chemical compounds: and the problem at present before the inquirer is, to devise such arrangements of molecules as shall answer the conditions alike of Chemistry and of Crystallography.

We now proceed to the Classificatory Sciences, of which Mineralogy is one, though hitherto by far the least successful.

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[6] See *Hist. Ind. Sc.* b. xv. c. vi.

BOOK VIII.

THE

PHILOSOPHY

OF THE

CLASSIFICATORY SCIENCES.

WHERE a certain apparent difference between things (although perhaps in itself of little moment) answers to we know not what number of other differences, pervading not only their known properties but properties yet undiscovered, it is not optional but imperative to recognise this difference as the foundation of a specific distinction.

JOHN S. MILL, *System of Logic*, b. 1, ch. vii. § 4.

BOOK VIII.

THE PHILOSOPHY OF THE CLASSIFICATORY SCIENCES.

CHAPTER I.

THE IDEA OF LIKENESS AS GOVERNING THE USE OF COMMON NAMES.

1. *Object of the Chapter.*—NOT only the Classificatory Sciences, but the application of names to things in the rudest and most unscientific manner, depends upon our apprehending them as *like* each other. We must therefore endeavour to trace the influence and operation of the Idea of Likeness in the common use of language, before we speak of the conditions under which it acquires its utmost exactness and efficacy.

It will be my object to show in this, as in previous cases, that the impressions of sense are apprehended by acts of the mind; and that these mental acts necessarily imply certain relations which may be made the subjects of speculative reasoning. We shall have, if we can, to seize and bring into clear view the principles which the relation of *like* and *unlike* involves, and the mode in which these principles have been developed.

2. *Unity of the Individual.*—But before we can attend to several things as like or unlike, we must be able to apprehend each of these by itself as *one thing.*

It may at first sight perhaps appear that this apprehension results immediately from the impressions on our senses, without any act of our thoughts. A very little attention, however, enables us to see that thus to single out special objects requires a mental operation as well as a sensation. How, for example, without an exertion of mental activity, can we see one tree, in a forest where there are many? We have, spread before us, a collection of colours and forms, green and brown, dark and light, irregular and straight: this is all that sensation gives or can give. But we associate one brown trunk with one portion of the green mass, excluding the rest, although the neighbouring leaves are both nearer in contiguity and more similar in appearance than is the stem. We thus have before us one tree; but this unity is given by the mind itself. We see the green and the brown, but we must *make* the tree before we can see *it*.

That this composition of our sensations so as to form *one thing* implies an act of our own, will perhaps be more readily allowed, if we once more turn our attention to the manner in which we sometimes attempt to imitate and record the objects of sight, by drawing. When we do this, as we have already observed, we mark this unity of each object, by drawing a line to separate the parts which we include from those which we exclude;—an *Outline*. This line corresponds to nothing which we see; the beginner in drawing has great difficulty in discerning it; he has in fact to make it. It is, as has been said by a painter of our own time[1], a fiction: but it is a fiction employed to mark a real act of the mind; to designate the singleness of the object in our conception. As we have said elsewhere, we see lines, but especially outlines, by mentally drawing them ourselves.

The same act of conception which the outline thus represents and commemorates in visible objects,—the same combination of sensible impressions into a unit,—is exercised also with regard to the objects of all

[1] Phillips *On Painting*,—Design.

our senses: and the singleness thus given to each object, is a necessary preliminary to its being named or represented in any other way.

But it may be said, Is it then by an arbitrary act of our own that we put together the branches of the same tree, or the limbs of the same animal? Have we equally the power and the right to make the branch of the fir a part of the neighbouring oak? Can we include in the outline of a man any object with which he happens to be in contact?

Such suppositions are manifestly absurd. And the answer is, that though we give unity to objects by an act of thought, it is not by an *arbitrary* act; but by a process subject to certain conditions;—to conditions which exclude such incongruous combinations as have just been spoken of.

What are these conditions which regulate our apprehension of an object as one?—which determine what portion of our impressions does, and what portion does not belong to the same thing?

3. *Condition of Unity.*—I reply, that the primary and fundamental condition is, that we must be able to make intelligible assertions respecting the object, and to entertain that belief of which assertions are the exposition. A tree *grows*, *sheds* its leaves in autumn, and *buds* again in the spring, *waves* in the wind, or *falls* before the storm. And to the tree belong all those parts which must be included in order that such declarations, and the thought which they convey, shall have a coherent and permanent meaning. Those are *its* branches which wave and fall with *its* trunk; those are *its* leaves which grow on *its* branches. The permanent connexions which we observe,—permanent, among unconnected changes which affect the surrounding appearances,—are what we bind together as belonging to one object. This permanence is the condition of our conceiving the object *as* one. The connected changes may always be described by means of assertions; and the connexion is seen in the identity of the subject of successive predications; in the possibility of applying many verbs to one substantive. We may

therefore express the condition of the unity of an object to be this: that *assertions concerning the object shall be possible:* or rather we should say, that the acts of belief which such assertions enunciate shall be possible.

It may seem to be superfluous to put in a form so abstract and remote, the grounds of a process apparently so simple as our conceiving an object to be one. But the same condition to which we have thus been led, as the essential principle of the unity of objects, namely, that propositions shall be possible, will repeatedly occur in the present chapter; and it may serve to illustrate our views, to show that this condition pervades even the simplest cases.

4. *Kinds.*—The mental synthesis of which we have thus spoken, gives us our knowledge of *individual* things; it enables me to apprehend that particular tree or man which I now see, or, by the help of memory, the tree or the man I saw yesterday. But the knowledge with which we have mainly here to do is not a knowledge of individuals but of kinds; of such classes as are indicated by common names. We have to make assertions concerning a tree or a man in general, without regarding what is peculiar to this man or that tree.

Now it is clear that certain individual objects are all called *man*, or all called *tree*, in virtue of some resemblance which they have. If we had not the power of perceiving in the appearances around us, likeness and unlikeness, we could not consider objects as distributed into kinds at all. The impressions of sense would throng upon us, but being uncompared with each other, they would flow away like the waves of the sea, and each vanish from our contemplation when the sensation faded. That we do apprehend surrounding objects as belonging to permanent kinds, as being men and horses, oaks and roses, arises from our having the idea of likeness, and from our applying it habitually, and so far as such a classification requires.

Not only can we employ the idea of likeness in this manner, but we apply it incessantly and universally to

the whole mass and train of our sensations. For we have no external sensations to which we cannot apply some language or other; and all language necessarily implies recognition of resemblances. We cannot call an object *green* or *round* without comparing in our thoughts its colour or its shape, with a shape and a colour seen in other objects. All our sensations, therefore, without any exception of kind or time, are subject to this constant process of classification; and the idea of likeness is perpetually operating to distribute them into kinds, at least so far as the use of language requires.

We come then again to the question, Upon what principle, under what conditions, is the Idea of Likeness thus operative? What are the limits of the classes thus formed? Where does that similarity end, which induces and entitles us to call a thing a *tree?* What universal rule is there for the application of common names, so that we may not apply them wrongly?

5. *Not made by Definitions.*—Perhaps some one might expect in answer to these inquiries a definition or a series of definitions;—might imagine that some description of a tree might be given which might show when the term was applicable and when it was not; and that we might construct a body of rules to which such descriptions must conform. But on consideration it will be clear that the real solution of our difficulty cannot be obtained in such a manner. For *first;* such descriptions must be given in words, and must therefore suppose that we have already satisfied ourselves how words are to be used. If we define a tree to be 'a living thing without the power of voluntary motion,' we shall be called upon to define 'a living thing;' and it is manifest that this renewal of the demand for definition might be repeated indefinitely; and, therefore, we cannot in this way come to a final principle. And in the *next* place, most of those who use language, even with great precision and consistency, would find it difficult or impossible to give good definitions even of a few of the general names which they use; and therefore their practice cannot be regulated by any

tacit reference to such definitions. That definitions of terms are of great use and importance in their right place, we shall soon see; but their place is not to regulate the use of common language.

What then, once more, is this regulative principle? What rules do men follow in the use of words, so as commonly to avoid confusion and ambiguity? How do they come to understand each other so well as they ordinarily do, respecting the limits of classes never defined, and which they cannot define? What is the common Convention, or Condition to which they conform?

6. *Condition of the Use of Terms.*—To this we reply, that the Condition which regulates the use of language, is, that it shall be capable of being used;—that is, that general assertions shall be possible. The term *tree* is applicable as far as it is useful in expressing our knowledge concerning trees:—thus we know that trees are fixed in the ground, have a solid stem, branches, leaves, and many other properties. With regard to all the objects which surround us, we have an immense store of knowledge of such properties, and we employ the names of the objects in such a manner as enables us to express these properties.

But the connexion of such properties is variable and indefinite. Some properties are constantly combined, others occasionally only. The leaves of different oaks resemble each other, the branches resemble far less, and may differ very widely. The term *oak* does not enable us to say that all oaks have straight branches or all crooked. Terms can only express properties as far as they are constant. Not only, therefore, the accumulation of a vast mass of knowledge of the properties and attributes of objects, but also an observation of the habitual *connexion* of such properties is needed, to direct us to the consistent application of terms:—to enable us to apply them so as to express truths. But here again we are largely provided with the requisite knowledge and observation by the common course of our existence. The unintermitting stream of experience supplies us with an incalculable

amount of such observed connexions. All men have observed that the associations of the same form of leaves are more constant than of the same form of branches;—that though persons walk in different attitudes, none go on all fours; and thus the term *oak* is so applied as to include those cases in which the leaves are alike in form though the branches be unlike; and though we should refuse to apply the term *man* to a class of creatures which habitually and without compulsion used four legs, we make no scruple of affixing it to persons of very different figures. The whole of human experience being composed of such observed connexions, we have thus materials even for the immense multiplicity of names which human language contains; all which names are, as we have said, regulated in their application by the condition of their expressing such experience.

Thus amid the countless combinations of properties and divisions of classes which the structure of language implies, scarcely any are arbitrary or capricious. A word which expressed a mere wanton collection of unconnected attributes could hardly be called a *word;* for of such a collection of properties no truth could be asserted, and the word would disappear, for want of some occasion on which it could be used. Though much of the fabric of language appears, not unnaturally, fantastical and purely conventional, it is in fact otherwise. The associations and distinctions of phraseology are not more fanciful than is requisite to make them correspond to the apparent caprices of nature or of thought; and though much in language may be called conventional, the conventions exist for the sake of expressing some truth or opinion, and not for their own sake. The principle, that *the condition of the use of terms is the possibility of general, intelligible, consistent assertions,* is true in the most complete and extensive sense.

7. *Terms may have different Uses.*—The Terms with which we are here most concerned are Names of Classes of natural objects; and when we say that the principle and the limit of such Names are their use in expressing propositions concerning the classes, it is

clear that much will depend on the kind of propositions which we mainly have to express: and that the same name may have different limits, according to the purpose we have in view. For example, is the *whale* properly included in the general term *fish?* When men are concerned in catching marine animals, the main features of the process are the same however the animals may differ; hence whales are classed with fishes, and we speak of the *whale-fishery*. But if we look at the analogies of organization, we find that, according to these, the whale is clearly not a fish, but a *beast*, (confining this term, for the sake of distinctness, to suckling beasts or *mammals*). In Natural History, therefore, the whale is not included among fish. The indefinite and miscellaneous propositions which language is employed to enunciate in the course of common practical life, are replaced by a more coherent and systematic collection of properties, when we come to aim at scientific knowledge. But we shall hereafter consider the principle of the classifications of Natural History; our present subject is the application of the Idea of Likeness in common practice and common language.

8. *Gradation of Kinds.*—Common names, then, include many individuals associated in virtue of resemblances, and of permanently connected properties; and such names are applicable as far as they serve to express such properties. These collections of individuals are termed *Kinds, Sorts, Classes.*

But this association of particulars is capable of degrees. As individuals by their resemblances form Kinds, so kinds of things, though different, may resemble each other so as to be again associated in a higher Class; and there may be several successive steps of such classification. *Man, horse, tree, stone*, are each a name of a Kind; but *animal* includes the two first and excludes the others; *living thing* is a term which includes *animal* and *tree* but not *stone; body* includes all the four. And such a subordination of kinds may be traced very widely in the arrangements of language.

The condition of the use of the wider is the same as that of the narrower Names of Classes;—they are good as far as they serve to express true propositions. In common language, though such an order of generality may in a variety of instances be easily discerned, it is not systematically and extensively referred to; but this subordination and graduated comprehensiveness is the essence of the methods and nomenclatures of Natural History, as we shall soon have to show.

But such subordination is not without its use, even in common cases, and when it is expressed in the terms of common language. Thus *organized body* is a term which includes plants and animals; *animal* includes beasts, birds, fishes; *beast* includes horses and dogs; *dogs*, again, are greyhounds, spaniels, terriers.

9. *Characters of Kinds.*—Now when we have such a Series of Names and Classes, we find that we take for granted irresistibly that each class has some *Character* which distinguishes it from other classes included in the superior division. We ask what kind of beast a dog is; what kind of animal a beast is; and we assume that such questions admit of answer;—that each kind has some mark or marks by which it may be described. And such descriptions may be given: an animal is an organized body *having sensation and volition;* man is a *reasonable* animal. Whether or no we assent to the exactness of these definitions, we allow the propriety of their form. If we maintain these definitions to be wrong, we must believe some others to be right, however difficult it may be to hit upon them. We entertain a conviction that there must be, among things so classed and named, a possibility of defining each.

Now what is the foundation of this postulate? What is the ground of this assumption, that there must exist a definition which we have never seen, and which perhaps no one has seen in a satisfactory form? The knowledge of this definition is by no means necessary to our using the word with propriety; for any one can make true assertions about dogs, but who can define a

dog? And yet if the definition be not necessary to enable us to use the word, why is it necessary at all? I allow that we possess an indestructible conviction that there must be such a character of each kind as will supply a definition; but I ask, on what this conviction rests.

I reply, that our persuasion that there must needs be characteristic marks by which things can be defined in words, is founded on the assumption of *the necessary possibility of reasoning.*

The reference of any object or conception to its class without definition, may give us a persuasion that it shares the properties of its class, but such classing does not enable us to reason upon those properties. When we consider man as an animal, we ascribe to him in thought the appetites, desires, affections, which we habitually include in our notion of animal: but except we have expressed these in some definition or acknowledged description of the term *animal*, we can make no use of the persuasion in ratiocination. But if we have described animals as 'being impelled to action by appetites and passions,' we can not only think, but say, 'man is an animal, and therefore he is impelled to act by appetites and passions.' And if we add a further definition, that 'man is a reasonable animal,' and if it appear that 'reason implies conformity to a rule of action,' we can then further infer that man's nature is to conform the results of animal appetite and passion to a rule of action.

The possibility of pursuing any such train of reasoning as this, depends on the definitions, of *animal* and of *man*, which we have introduced; and the possibility of reasoning concerning the objects around us being inevitably assumed by us from the constitution of our nature, we assume consequently the possibility of such definitions as may thus form part of our deduction, and the existence of such defining characters.

10. *Difficulty of Definitions.*—But though men are, on such grounds, led to make constant and importunate *demands* for definitions of the terms which they employ in their speculations, they are, in fact, far

from being able to carry into complete effect the postulate on which they proceed, that they must be able to find definitions which by logical consequence shall lead to the truths they seek. The postulate overlooks the process by which our classes of things are formed and our names applied. This process consisting, as we have already said, in observing permanent connexions of properties, and in fixing them by the attribution of names, is of the nature of the process of Induction, of which we shall afterwards have to speak. And the postulate is so far true, that this process of induction being once performed, its result may usually be expressed by means of a few definitions, and may thus lead by a deduction to a train of real truths.

But in the subjects where we principally find such a subordination of classes as we have spoken of, this process of deduction is rarely of much prominence: for example, in the branches of natural history. Yet it is in these subjects that the existence and importance of these characteristic marks, which we have spoken of, principally comes into view. In treating of these marks, however, we enter upon methods which are technical and scientific, not popular and common. And before we make this transition, we have a remark to make on the manner in which writers, without reference to physics or natural history, have spoken of kinds, their subordination, and their marks.

11. '*The Five Words.*'—These things,—the Nature and Relations of Classes,—were, in fact, the subjects of minute and technical treatment by the logicians of the school of Aristotle. Porphyry wrote an Introduction to the *Categories* of that philosopher, which is entitled *On the Five Words.* The 'Five Words' are *Genus, Species, Difference, Property, Accident.* Genus and Species are superior and inferior classes, and are stated[2] to be capable of repeated subordination. The 'most

[2] Porphyr. *Isagog.* c. 23.

general Genus' is the widest class; the 'most special Species' the narrowest. Between these are intermediate classes, which are Genera with regard to those below, and Species with regard to those above them. Thus Being is the most general Genus; under this is Body; under Body is Living Body; under this again Animal; under Animal is Rational Animal, or Man; under Man are Socrates and Plato, and other individual men.

The *Difference* is that which is added to the genus to make the species; thus Rational is the Difference by which the genus Animal is made the species Man; the Difference in this Technical sense is the 'Specific,' or species-making Difference[3]. It forms the Definition for the purposes of logic, and corresponds to the 'Character' (specific or generic) of the Natural Historians. Indeed several of them, as, for instance, Linnæus, in his *Philosophia Botanica*, always call these Characters the *Difference*, by a traditional application of the Peripatetic terms of art.

Of the other two words, the Property is that which though not employed in defining the class, belongs to every part of it[4]: it is, 'What happens to all the class, to it alone, and at all times; as *to be capable of laughing* is a Property of man.'

The Accident is that which may be present and absent without the destruction of the subject, as to sleep is an Accident (a thing which happens) to man.

I need not dwell further on this system of technicalities. The most remarkable points in it are those which I have already noticed; the doctrine of the successive Subordination of genera, and the fixing attention upon the Specific Difference. These doctrines, though invented in order to make reasoning more systematic, and at a period anterior to the existence of any Classificatory Science, have, by a curious contrast with the intentions of their founders, been of scarcely

[3] εἰδοποιός.

[4] *Isagog.* c. 4.

any use in sciences of *Reasoning*, but have been amply applied and developed in the *Natural History* which arose in later times.

We must now treat of the principles on which this science (Natural History) proceeds, and explain what peculiar and technical processes it employs in addition to those of common thought and common language.

CHAPTER II.

The Methods of Natural History, as regulated by the Idea of Likeness.

Sect. I.—*Natural History in general.*

1. *Idea of Likeness in Natural History.*—The various branches of Natural History, in so far as they are classificatory sciences merely, and do not depend upon physiological views, rest upon the same Idea of Likeness which is the ground of the application of the names, more or less general, of common language. But the nature of science requires that, for her purposes, this Idea should be applied in a more exact and rigourous manner than in its common and popular employment; just as occurs with regard to the other Ideas on which science is founded;—for instance, as the idea of space gives rise, in popular use, to the relations implied in the prepositions and adjectives which refer to position and form, and in its scientific development gives rise to the more precise relations of geometry.

The way in which the Idea of Likeness has been applied, so as to lead to the construction of a science, is best seen in Botany: for, in the Classification of Animals, we are inevitably guided by a consideration of the *function* of parts; that is, by an idea of *purpose*, and not of likeness merely: and in Mineralogy, the attempts at classification on the principles of Natural History have been hitherto very imperfectly successful. But in Botany we have an example of a branch of knowledge in which systematic classification has been effected with great beauty and advantage; and in which the peculiarities and principles on which such

classification must depend have been carefully studied. Many of the principal botanists, as Linnæus, Adanson, Decandolle, have not only practically applied, but have theoretically enunciated, what they held to be the sound maxims of classificatory science: and have thus enabled us to place before the reader with confidence the philosophy of this kind of science.

2. *Condition of its Use.*—We may begin by remarking that the Idea of Likeness, in its systematic employment, is governed by the same principle which we have already spoken of as regulating the distribution of things into kinds, and the assignment of names in unsystematic thought and speech; namely, the condition that *general propositions shall be possible.* But as in this case the propositions are to be of a scientific form and exactness, the likeness must be treated with a corresponding precision; and its consequences traced by steady and distinct processes. Naturalists must, for their purposes, employ the resemblances of objects in a technical manner. This technical process may be considered as consisting of three steps;—The fixation of the resemblances; The use of them in making a classification; The means of applying the classification. These three steps may be spoken of as the *Terminology*, the *Plan of the System*, and the *Scheme of the Characters.*

SECT. II.—*Terminology*[1].

3. *Terminology* signifies the collection of *terms*, or technical words, which belong to the science. But in fixing the meaning of the terms, at least of the descriptive terms, we necessarily fix, at the same time, the perceptions and notions which the terms are to

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1 Decandolle and others use the term *Glossology* instead of *Terminology*, to avoid the blemish of a word compounded of two parts taken from different languages. The convenience of treating the termination *ology* (and a few other parts of compounds) as not restricted to Greek combinations, is so great, that I shall venture, in these cases, to disregard this philological scruple.

convey; and thus the Terminology of a classificatory science exhibits the elements of its substance as well as of its language. A large but indispensable part of the study of botany (and of mineralogy and zoology also,) consists in the acquisition of the peculiar vocabulary of the science.

The meaning of technical terms can be fixed in the first instance only by convention, and can be made intelligible only by presenting to the senses that which the terms are to signify. The knowledge of a colour by its name can only be taught through the eye. No description can convey to a hearer what we mean by *apple-green* or *French grey*. It might, perhaps, be supposed that, in the first example, the term *apple*, referring to so familiar an object, sufficiently suggests the colour intended. But it may easily be seen that this is not true; for apples are of many different hues of green, and it is only by a conventional selection that we can appropriate the term to one special shade. When this appropriation is once made, the term refers to the sensation, and not to the parts of this term; for these enter into the compound merely as a help to the memory, whether the suggestion be a natural connexion as in 'apple-green,' or a casual one as in 'French grey.' In order to derive due advantage from technical terms of this kind, they must be associated *immediately* with the perception to which they belong; and not connected with it through the vague usages of common language. The memory must retain the sensation; and the technical word must be understood as directly as the most familiar word, and more distinctly. When we find such terms as *tin-white* or *pinchbeck-brown*, the metallic colour so denoted ought to start up in our memory without delay or search.

This, which it is most important to recollect with respect to the simpler properties of bodies, as colour and form, is no less true with respect to more compound notions. In all cases the term is fixed to a peculiar meaning by convention; and the student, in order to use the word, must be completely familiar with the convention, so that he has no need to frame

conjectures from the word itself. Such conjectures would always be insecure, and often erroneous. Thus the term *papilionaceous*, applied to a flower, is employed to indicate, not only a resemblance to a butterfly, but a resemblance arising from five petals of a certain peculiar shape and arrangement; and even if the resemblance to a butterfly were much stronger than it is in such cases, yet if it were produced in a different way, as, for example, by one petal, or two only, instead of a 'standard,' two 'wings,' and a 'keel' consisting of two parts more or less united into one, we should no longer be justified in speaking of it as a 'papilionaceous' flower.

The formation of an exact and extensive descriptive language for botany has been executed with a degree of skill and felicity, which, before it was attained, could hardly have been dreamt of as attainable. Every part of a plant has been named; and the form of every part, even the most minute, has had a large assemblage of descriptive terms appropriated to it, by means of which the botanist can convey and receive knowledge of form and structure, as exactly as if each minute part were presented to him vastly magnified. This acquisition was part of the Linnæan Reform, of which we have spoken in the *History*. 'Tournefort,' says Decandolle[2], 'appears to have been the first who really perceived the utility of fixing the sense of terms in such a way as always to employ the same word in the same sense, and always to express the same idea by the same word; but it was Linnæus who really created and fixed this botanical language, and this is his fairest claim to glory, for by this fixation of language he has shed clearness and precision over all parts of the science.'

It is not necessary here to give any detailed account of the terms of botany. The fundamental ones have been gradually introduced, as the parts of plants were more carefully and minutely examined. Thus the flower was successively distinguished into the *calyx*,

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[2] *Theor. Elem.* p. 327.

the *corolla*, the *stamens*, and the *pistils:* the sections of the corolla were termed *petals* by Columna; those of the calyx were called *sepals* by Necker[3]. Sometimes terms of greater generality were devised; as *perianth* to include the calyx and corolla, whether one or both of these were present[4]; *pericarp* for the part inclosing the grain, of whatever kind it be, fruit, nut, pod, &c. And it may easily be imagined that descriptive terms may, by definition and combination, become very numerous and distinct. Thus leaves may be called *pinnatifid*[5], *pinnatipartite, pinnatisect, pinnatilobate, palmatifid, palmatipartite,* &c., and each of these words designates different combinations of the modes and extent of the divisions of the leaf with the divisions of its outline. In some cases arbitrary numerical relations are introduced into the definition: thus a leaf is called *bilobate*[6] when it is divided into two parts by a notch; but if the notch go to the middle of its length, it is *bifid;* if it go near the base of the leaf, it is *bipartite;* if to the base, it is *bisect.* Thus, too, a pod of a cruciferous plant is a *silica*[7] if it be four times as long as it is broad, but if it be shorter than this it is a *silicula.* Such terms being established, the form of the very complex leaf or frond of a fern is exactly conveyed by the following phrase: 'fronds rigid pinnate, pinnæ recurved subunilateral pinnatifid, the segments linear undivided or bifid spinuloso-serrate[8].'

Other characters, as well as form, are conveyed with the like precision: Colour by means of a classified scale of colours, as we have seen in speaking of the Measures of Secondary Qualities; to which, however, we must add, that the naturalist employs arbitrary names, (such as we have already quoted,) and not mere numerical exponents, to indicate a certain number of

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[3] Decandolle, 329.

[4] For this Erhart and Decandolle use *Perigone.*

[5] Dec. 318.

[6] *Ib.* 493.

[7] *Ib.* 422.

[8] Hooker, *Brit. Flo.* p. 45̣. *Hymenophyllum Wilsoni,* Scottish filmy-fern, abundant in the highlands of Scotland and about Killarney.

selected colours. This was done with most precision by Werner, and his scale of colours is still the most usual standard of naturalists. Werner also introduced a more exact terminology with regard to other characters which are important in mineralogy, as lustre, hardness. But Mohs improved upon this step by giving a numerical scale of hardness, in which *talc* is 1, *gypsum* 2, *calc spar* 3, and so on, as we have already explained in the History of Mineralogy. Some properties, as specific gravity, by their definition give at once a numerical measure; and others, as crystalline form, require a very considerable array of mathematical calculation and reasoning, to point out their relations and gradations. In all cases the features of likeness in the objects must be rightly apprehended, in order to their being expressed by a distinct terminology. Thus no terms could describe crystals for any purpose of natural history, till it was discovered that in a class of minerals the proportion of the faces might vary, while the angle remained the same. Nor could crystals be described so as to distinguish species, till it was found that the derived and primitive forms are connected by very simple relations of space and number. The discovery of the mode in which characters must be apprehended so that they may be considered as *fixed* for a class, is an important step in the progress of each branch of Natural History; and hence we have had, in the History of Mineralogy and Botany, to distinguish as important and eminent persons those who made such discoveries, Romé de Lisle and Haüy, Cesalpinus and Gesner.

By the continued progress of that knowledge of minerals, plants, and other natural objects, in which such persons made the most distinct and marked steps, but which has been constantly advancing in a more gradual and imperceptible manner, the most important and essential features of similarity and dissimilarity in such objects have been selected, arranged, and fitted with names; and we have thus in such departments, systems of Terminology which fix our attention upon the resemblances which it is proper to consider, and

enable us to convey them in words. We have now to speak of the mode in which such resmblances have been employed in the construction of a Systematic Classification.

Sect. III. *The Plan of the System.*

4. The collection of sound views and maxims by which the resemblances of natural objects are applied so as to form a scientific classification, is a department of the philosophy of natural history which has been termed by some writers (as Decandolle), *Taxonomy*, as containing the *Laws* of the *Taxis* (*arrangement*). By some Germans this has been denominated *Systematik;* if we could now form a new substantive after the analogy of the words *Logick*, *Rhetorick*, and the like, we might call it *Systematick*. But though our English writers commonly use the expression *Systematical Botany* for the Botany of Classification, they appear to prefer the term *Diataxis* for the method of constructing the classification. The rules of such a branch of science are curious and instructive.

In framing a Classification of objects we must attend to their resemblances and differences. But here the question occurs, to *what* resemblances and differences? for a different selection of the points of resemblance would give different results: a plant frequently agrees in leaves with one group of plants, in flowers with another. Which set of characters are we to take as our guide?

The view already given of the regulative principle of all classification, namely, that it must enable us to assert true and general propositions, will obviously occur as applicable here. The object of a scientific Classification is to enable us to enunciate scientific truths: we must therefore classify according to those resemblances of objects (plants or any others) which bring to light such truths.

But this reply to the inquiry, 'On what characters of resemblance we are to found our system,' is still too general and vague to be satisfactory. It carries us,

however, as far as this;—that since the truths we are to attend to are scientific truths, governed by precise and homogeneous relations, we must not found our scientific Classification on casual, indefinite, and unconnected considerations. We must not, for instance, be satisfied with dividing plants, as Dioscorides does, into *aromatic*, *esculent*, *medicinal* and *vinous;* or even with the long prevalent distribution into *trees*, *shrubs*, and *herbs;* since in these subdivisions there is no consistent principle.

5. *Latent Reference to Natural Affinity.*—But there may be several kinds of truths, all exact and coherent, which may be discovered concerning plants or any other natural objects; and if this should be the case, our rule leaves us still at a loss in what manner our classification is to be constructed. And, historically speaking, a much more serious inconvenience has been this;—that the task of classification of plants was necessarily performed when the general laws of their form and nature were very little known; or rather, when the existence of such laws was only just beginning to be discerned. Even up to the present day, the general propositions which botanists are able to assert concerning the structure and properties of plants, are extremely imperfect and obscure.

We are thus led to this conclusion:—that the Idea of Likeness could not be applied so as to give rise to a scientific Classification of plants, till considerable progress was made in studying the general relations of vegetable form and life; and that the selection of the resemblances which should be taken into account, must depend upon the nature of the relations which were then brought into view.

But this amounts to saying that, in the consideration of the Classification of vegetables, other Ideas must be called into action as well as the Idea of Likeness. The additional general views to which the more intimate study of plants leads, must depend, like all general truths, upon some regulating Idea which gives unity to scattered facts. No progress could be made in botanical knowledge without the

operation of such principles: and such additional Ideas must be employed, besides those of mere likeness and unlikeness, in order to point out that Classification which has a real scientific value.

Accordingly, in the classificatory sciences, Ideas other than Likeness do make their appearance. Such Ideas in botany have influenced the progress of the science, even before they have been clearly brought into view. We have especially the Idea of Affinity, which is the basis of all Natural Systems of Classification, and which we shall consider in a succeeding chapter. The assumption that there *is* a Natural System, an assumption made by all philosophical botanists, implies a belief in the existence of Natural Affinity, and is carried into effect by means of principles which are involved in that Idea. But as the formation of all systems of classification must involve, in a great degree, the Idea of Resemblance and Difference, I shall first consider the effect of that Idea, before I treat specially of Natural Affinity.

6. *Natural Classes.*—Many attempts were made to classify vegetables before the rules which govern a natural system were clearly apprehended. Botanists agree in esteeming some characters as of more value than others, before they had agreed upon any general rules or principles for estimating the relative importance of the characters. They were convinced of the necessity of adding other considerations to that of Resemblance, without seeing clearly what these others ought to be. They aimed at a Natural Classification, without knowing distinctly in what manner it was to be Natural.

The attempts to form *Natural Classes,* therefore, in the first part of their history, belong to the Idea of Likeness, though obscurely modified, even from an early period, by the Ideas of Affinity, and even of Function and of Development. Hence Natural Classes may, to a certain extent, be treated of in this place.

Natural Classes are opposed to Artificial Classes which are understood to be regulated by an *assumed*

character. Yet no classes can be so absolutely Artificial in this sense, as to be framed upon characters *arbitrarily* assumed; for instance, no one would speak of a class of shrubs defined by the circumstance of each having a hundred leaves: for of such a class no assertion could be made, and therefore the class could never come under our notice. In what sense then are Artificial Classes to be understood, as opposed to Natural?

7. *Artificial Classes.*—To this question, the following is the answer. When Natural Classes of a certain small extent have been formed, a system may be devised which shall be regulated by a few selected characters, and which shall not dissever these small Natural Classes, but conform to them as far as they go. If these selected characters be then made absolute and imperative, and if we abandon all attempt to obtain Natural Classes of any higher order and wider extent, we form an Artificial System.

Thus in the Linnæan System of Botanical Classification, it is assumed that certain natural groups, namely, Species and Genera, are established; it is conceived, moreover, that the division of Classes according to the number of stamens and of pistils does not violate the natural connexions of Species and Genera. This arrangement, according to the number of stamens and pistils, (further modified in certain cases by other considerations,) is then made the ground of all the higher divisions of plants, and thus we have an Artificial System.

It has been objected to this view, that the Linnæan Artificial System does not in all cases respect the boundaries of genera, but would, if rigorously applied, distribute the species of the same genus into different artificial classes; it would divide, for instance, the genera *Valeriana*, *Geranium*[9], &c. To this we must reply, that so far as the Linnæan System does this, it is an imperfect Artificial System. Its great merit is in its making such a disjunction in comparatively so

[9] Decand. *Theor. Elem.* p. 45.

few cases; and in the artificial characters being, for the most part, obvious and easily applied.

8. *Are Genera Natural?*—It has been objected also that Genera are not Natural groups. Linnæus asserts in the most positive manner that they are[10]. On which Adanson observes[11], 'I know not how any Botanist can maintain such a thesis: that which is certain is, that up to the present time no one has been able to prove it, nor to give an exact definition of a natural genus, but only of an artificial.' He then brings several arguments to confirm this view.

But we are to observe, in answer to this, that Adanson improperly confounds the recognition of the existence of a natural group with the invention of a technical mark or definition of it. Genera are groups of species associated in virtue of natural affinity, of general resemblance, of real propinquity: of such groups, certain selected characters, one or few, may usually be discovered, by which the species may be referred to their groups. These Artificial characters do not constitute, but indicate the genus: they are the *Diagnosis*, not the basis of the *Diataxis:* and they are always subject to be rejected, and to have others substituted for them, when they violate the natural connexion of species which a minute and enlarged study discovers.

It is, therefore, no proof that Genera are not Natural, to say that their artificial characters are different in different systems. Such characters are only different attempts to confine the variety of nature within the limits of definition. Nor is it sufficient to say that these groups themselves are different in different writers; that some botanists make genera what others make only species; as *Pedicularis, Rhinanthus, Euphrasia, Antirrhinum*[12]. This discrepancy shows only that the natural arrangement is not yet completely known, even in the smaller groups; a conclusion to which we need not refuse our assent. But in opposi-

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[10] *Phil. Bot.* Art. 165. [11] *Famille de Ph.* Pref. cv.
[12] Adanson, p. cvi.

tion to these negatives, the manner in which Genera have been established proves that they are regulated by the principle of being natural, and by that alone. For they are not formed according to any *à priori* rule. The Botanist does not take any selected or arbitrary part or parts of the plants, and marshal his genera according to the differences of this part. On the contrary, the divisions of genera are sometimes made by means of the flower; sometimes by means of the fruit: the anthers, the stamens, the seeds, the pericarp, and the most varied features of these parts, are used in the most miscellaneous and unsystematic manner. Linnæus has indeed laid down a maxim that the characteristic differences of genera must reside in the fructification[13]: but Adanson has justly remarked[14], that an arbitrary restriction like this makes the groups artificial: and that in some families other characters are more essential than those of the fructification; as the leaves in the families of *Aparineæ* and *Leguminosæ*, and the disposition of the flowers in *Labiatæ*. And Naturalists are so far from thinking it sufficient to distribute species into genera by *arbitrary* marks, that we find them in many cases lamenting the absence of good *natural* marks: as in the families of *Umbelliferæ*, where Linnæus declared that any one who could find good characters of genera would deserve great admiration, and where it is only of late that good characters have been discovered and the arrangement settled[15] by means principally of the ribs of the fruit[16].

It is thus clear that Genera are not established on any assumed or preconceived basis. What, then, is the principle which regulates botanists when they try to fix genera? What is the arrangement which they thus wish for, without being able to hit upon it? What is the tendency which thus drives them from the corolla to the anthers, from the flower to the fruit,

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13 *Phil. Bot.* Art. 162.

14 Adanson, Pref. p. cxx.

15 Lindley, *Nat. Syst.* p. 5.

16 In like manner we find Cuvier saying of Rondelet that he has 'un *sentiment* très vrai des genres.' *Hist. Ichth.* p. 39.

from the fructification to the leaves? It is plain that they seek something, not of their own devising and creating;—not anything merely conventional and systematic; but something which they conceive to exist in the relations of the plants themselves;—something which is without the mind, not within;—in nature, not in art;—in short, a Natural Order.

Thus the regulative principle of a Genus, or of any other natural group is, that it is, or is supposed to be, natural. And by reference to this principle as our guide, we shall be able to understand the meaning of that indefiniteness and indecision which we frequently find in the descriptions of such groups, and which must appear so strange and inconsistent to any one who does not suppose these descriptions to assume any deeper ground of connexion than an arbitrary choice of the botanist. Thus in the family of the Rose-tree, we are told that the *ovules* are *very rarely* erect[17], the *stigmata* are *usually* simple. Of what use, it might be asked, can such loose accounts be? To which the answer is, that they are not inserted in order to distinguish the species, but in order to describe the family, and the total relations of the ovules and of the stigmata of the family are better known by this general statement. A similar observation may be made with regard to the Anomalies of each group, which occur so commonly, that Mr. Lindley, in his *Introduction to the Natural System of Botany*, makes the 'Anomalies' an article in each Family. Thus, part of the character of the Rosaceæ is that they have alternate *stipulate* leaves, and that the *albumen* is *obliterated:* but yet in *Lowea*, one of the genera of this family, the stipulæ are *absent;* and the albumen is *present* in another, *Neillia*. This implies, as we have already seen, that the artificial character (or *diagnosis* as Mr. Lindley calls it) is imperfect. It is, though very nearly, yet not exactly, commensurate with the natural group: and hence, in certain cases, this character is made to yield to the general weight of natural affinities.

[17] Lindley, *Nat. Syst.* p. 81.

9. *Difference of Natural History and Mathematics.* —These views,—of classes determined by characters which cannot be expressed in words,—of propositions which state, not what happens in all cases, but only usually,—of particulars which are included in a class though they transgress the definition of it, may very probably surprise the reader. They are so contrary to many of the received opinions respecting the use of definitions and the nature of scientific propositions, that they will probably appear to many persons highly illogical and unphilosophical. But a disposition to such a judgment arises in a great measure from this; —that the mathematical and mathematico-physical sciences have, in a great degree, determined men's views of the general nature and form of scientific truth; while Natural History has not yet had time or opportunity to exert its due influence upon the current habits of philosophizing. The apparent indefiniteness and inconsistency of the classifications and definitions of Natural History belongs, in a far higher degree, to all other except mathematical speculations: and the modes in which approximations to exact distinctions and general truths have been made in Natural History, may be worthy our attention, even for the light they throw upon the best modes of pursuing truth of all kinds.

10. *Natural Groups given by Type not by Definition.* —The further development of this suggestion must be considered hereafter. But we may here observe, that though in a Natural Group of objects a definition can no longer be of any use as a regulative principle, classes are not, therefore, left quite loose, without any certain standard or guide. The class is steadily fixed, though not precisely limited; it is given, though not circumscribed; it is determined, not by a boundary line without, but by a central point within; not by what it strictly excludes, but by what it eminently includes; by an example, not by a precept; in short, instead of Definition we have a *Type* for our director.

A Type is an example of any class, for instance, a species of a genus, which is considered as eminently

possessing the characters of the class. All the species which have a greater affinity with this Type-species than with any others, form the genus, and are ranged about it, deviating from it in various directions and different degrees. Thus a genus may consist of several species, which approach very near the type, and of which the claim to a place with it is obvious; while there may be other species which straggle further from this central knot, and which yet are clearly more connected with it than with any other. And even if there should be some species of which the place is dubious, and which appear to be equally bound by two generic types, it is easily seen that this would not destroy the reality of the generic groups, any more than the scattered trees of the intervening plain prevent our speaking intelligibly of the distinct forests of two separate hills.

The Type-species of every genus, the Type-genus of every family, is, then, one which possesses all the characters and properties of the genus in a marked and prominent manner. The Type of the Rose family has alternate stipulate leaves, wants the albumen, has the ovules not erect, has the stigmata simple, and besides these features, which distinguish it from the exceptions or varieties of its class, it has the features which make it prominent in its class. It is one of those which possess clearly several leading attributes; and thus, though we cannot say of any one genus that it *must* be the Type of the family, or of any one species that it *must* be the Type of the genus, we are still not wholly to seek: the Type must be connected by many affinities with most of the others of its group; it must be near the center of the crowd, and not one of the stragglers.

11. It has already been repeatedly stated, as the great rule of all classification, that the classification must serve to assert general propositions. It may be asked *what* propositions we are able to enunciate by means of such classifications as we are now treating of. And the answer is, that the collected knowledge of the characters, habits, properties, organization, and func-

tions of these groups and families, as it is found in the best botanical works, and as it exists in the minds of the best botanists, exhibits to us the propositions which constitute the science, and to the expression of which the classification is to serve. All that is not strictly definition, that is, all that is not artificial character, in the descriptions of such classes, is a statement of truths, more or less general, more or less precise, but making up, together, the positive knowledge which constitutes the science. As we have said, the consideration of the properties of plants in order to form a system of classification, has been termed Taxonomy, or the Systematick of Botany; all the parts of the descriptions, which, taking the system for granted, convey additional information, are termed the *Physiography* of the science; and the same terms may be applied in the other branches of Natural History.

12. *Artificial and Natural Systems.*—If I have succeeded in making it apparent that an artificial system of characters necessarily implies natural classes which are not severed by the artificial marks, we shall now be able to compare the nature and objects of the Artificial and Natural Systems; points on which much has been written in recent times.

The Artificial System is one which is, or professes to be, entirely founded upon marks selected according to the condition which has been stated, of not violating certain narrow natural groups; namely in the Linnæan system, the natural genera of plants. The marks which form the basis of the system, being thus selected, are applied rigorously and universally without any further regard to any other characters or indications of affinity. Thus in the Linnæan system, which depends mainly on the number of male organs or stamens, and on the number of female organs or styles, the largest divisions, or the *Classes*, are arranged according to the number of the stamens, and are *monandria*, *diandria*, *triandria*, *tetrandria*, *pentandria*, *hexandria*, and so on: the names being formed of the Greek numerical words, and of the word which implies *male.* And the Orders of each of these Classes are distin-

guished by the number of styles, and are called *monogynia, digynia, trigynia,* and so on, the termination of these words meaning *female.* And so far as this numerical division and subdivision go on, the system is a rigorous system, and strictly artificial.

But the condition that the artificial system shall leave certain natural affinities untouched, makes it impossible to go through the vegetable kingdom by a method of mere numeration of stamens and styles. The distinction of flowers with twenty and with thirty stamens is not a fixed distinction: flowers of one and the same kind, as roses, have, some fewer than the former, some more than the latter number. The Artificial System, therefore, must be modified. And there are various relations of connexion and proportion among the stamina which are more permanent and important than their mere number. Thus flowers with two longer and two shorter stamens are not placed in the class *tetrandria,* but are made a separate class *didynamia;* those with four longer and two shorter are in like manner *tetradynamia,* not *hexandria;* those in wich the filaments are bound into two bundles are *diadelphia.* All these and other classes are deviations from the plan of the earlier Classes, and are so far defects of the artificial system; but they are deviations requisite in order that the system may leave a basis of natural groups, without which it would not be a System of *Vegetables.* And as the division is still founded on some properties of the stamens, it combines not ill with that part of the system which depends on the number of them. The Classes framed in virtue of these various considerations make up an Artificial System which is tolerably coherent.

'But since the Artificial System thus regards natural groups, in what does it differ from a Natural System?' It differs in this:—That though it allows certain subordinate natural groups, it merely allows *these,* and does not endeavour to ascend to any wider natural groups. It takes all the *higher* divisions of its scheme from its artificial characters, its stamens and pistils, without looking to any natural affinities. It

accepts natural *Genera*, but it does not seek natural *Families*, or Orders, or Classes. It *assumes* natural groups, but does not *investigate* any; it forms wider and higher groups, but professes to frame them arbitrarily.

But then, on the other hand, the question occurs, 'This being the case, what can be the use of the Artificial System?' If its characters, in the higher stages of classification, be arbitrary, how can it lead us to the natural relations of plants? And the answer is, that it does so in virtue of the original condition, that there shall be certain natural relations which the artificial system shall not transgress; and that its use arises from the facility with which we can follow the artificial arrangement as far as it goes. We can count the stamens and pistils, and thus we know the Class and Order of our plant; and we have then to discover its Genus and Species by means less symmetrical but more natural. The Artificial System, though arbitrary in a certain degree, brings us to a Class in which the whole of each Genus is contained, and there we can find the proper Genus by a suitable method of seeking. No Artificial System can conduct us into the extreme of detail, but it can place us in a situation where the detail is within our reach. We cannot find the house of a foreign friend by its latitude and longitude; but we may be enabled, by a knowledge of the latitude and longitude, to find the city in which he dwells, or at least the island; and we then can reach his abode by following the road or exploring the locality. The Artificial System is such a method of travelling by latitude and longitude; the Natural System is that which is guided by a knowledge of the country.

The Natural System, then, is that which endeavours to arrange by the natural affinities of objects; and more especially, which attempts to ascend from the lower natural groups to the higher; as for example from genera to natural families, orders, and classes. But as we have already hinted, these expressions of natural affinities, natural groups, and the like, when

considered in reference to the idea of resemblance alone, without studying analogy or function, are very vague and obscure. We must notice some of the attempts which were made under the operation of this imperfect view of the subject.

SECT. IV.—*Modes of framing Natural Systems.*

13. Decandolle[18] distinguishes the attempts at Natural Classifications into three sorts: those of *blind trial* (*tâtonnement*), those of *general comparison*, and those of *subordination of characters.* The two former do not depend distinctly upon any principle, except resemblance; the third refers us to other views, and must be considered in a future chapter.

Method of Blind Trial.—The notion of the existence of natural classes dependent on the general resemblance of plants,—of an affinity showing itself in different parts and various ways,—though necessarily somewhat vague and obscure, was acted upon at an early period, as we have seen in the formation of genera; and was enunciated in general terms soon after. Thus Magnolius[19] says that he discerns in plants an affinity, by means of which they may be arranged in families: 'Yet it is impossible to obtain from the fructification alone the Characters of these families; and I have therefore chosen those parts of plants in which the principal characteristic marks are found, as the root, the stem, the flower, the seed. In some plants there is even a certain resemblance; an affinity which does not consist in the parts considered separately, but in their totality; an affinity which may be felt but not expressed; as we see in the families of agrimonies and cinquefoils, which every botanist will judge to be related, though they differ by their roots, their leaves, their flowers, and their seeds.'

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18 *Theor. Elem.* art. 41.

19 Dec. *Theor. Elem.* art. 42. Petri Magnoli, *Prodromus Hist. Gen. Plant.* 1689.

This obscure feeling of a resemblance on the whole, an affinity of an indefinite kind, appears fifty years later in Linnæus's attempts. 'In the Natural Classification,' he says[20], 'no *à priori* rule can be admitted, no part of the fructification can be taken exclusively into consideration; but only the simple symmetry of all its parts.' Hence though he proposed Natural Families, and even stated the formation of such Families to be the first and last object of all Methods, he never gave the Characters of those groups, or connected them by any method. He even declared it to be impossible to lay down such a system of characters. This persuasion was the result of his having refused to admit into his mind any Idea more profound than that notion of Resemblance of which he had made so much and such successful use; he would not attempt to unravel the Ideas of Symmetry and of Function on which the clear establishment of natural relations must depend. He even despised the study of the inner organization of plants; and reckoned[21] the *Anatomici*, who studied the anatomy and physiology of plants and the laws of vegetation, among the *Botanophili*, the mere amateurs of his science.

The same notion of general resemblance and affinity, accompanied with the same vagueness, is to be found in the writer who least participated in the general admiration of Linnæus, Buffon. Though it was in a great measure his love of higher views which made him dislike what he considered the pedantry of the Swedish school, he does not seem to have obtained a clearer sight of the principle of the natural method than his rival, except that he did not restrict his Characters to the fructification. Things must be arranged by their resemblances and differences, (he says in 1750[22],) 'but the resemblances and differences must be taken not from one part but from the whole; and we must attend to the form, the size, the habit, the number and position of the parts, even the substance

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[20] Dec. *Theor. Elem.* art. 42. [12] *Phil. Bot.* s. 44.

[22] Adanson, p. clvi. Buffon, *Hist. Nat.* t. i. p. 21.

of the part; and we must make use of these elements in greater or smaller number, as we have need.'

14. *Method of General Comparison.*—A countryman of Buffon, who shared with him his depreciating estimate of the Linnæan system, and his wish to found a natural system upon a broader basis, was Adanson; and he invented an ingenious method of apparently avoiding the vagueness of the practice of following the general feeling of resemblance. This method consisted in making many Artificial Systems, in each of which plants were arranged by some one part; and then collecting those plants which came near each other in the greatest number of those Artificial Systems, as plants naturally the most related. Adanson gives an account[23] of the manner in which this system arose in his mind. He had gone to Senegal, animated by an intense zeal for natural history; and there, amid the luxuriant vegetation of the torrid zone, he found that the methods of Linnæus and Tournefort failed him altogether as means of arranging his new botanical treasures. He was driven to seek a new system. 'For this purpose,' he says, 'I examined plants in all their parts, without omitting any, from the roots to the embryo, the folding of the leaves in the bud, their mode of sheathing[24], the situation and folding of the embryo and of its radicle in the seed, relatively to the fruit; in short, a number of particulars which few botanists notice. I made in the first place a complete description of each plant, putting each of its parts in separate articles, in all its details; when new species occurred I put down the points in which they differed, omitting those in which they agreed. By means of the aggregate of these comparative descriptions, I perceived that plants arranged themselves into classes or families which could not be artificial or arbitrary, not being founded upon one or two parts, which might change at certain limits, but on all the parts; so that the disproportion of one of these parts was corrected and balanced by the introduction of another.' Thus the principle of Resemblance

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[23] Pref. p. clvii. [24] 'Leur manière de s'engainer.'

was to suffice for the general arrangement, not by means of a new principle, as Symmetry or Organization, which should regulate its application, but by a numeration of the peculiarities in which the resemblance consisted.

The labour which Adanson underwent in the execution of this thought was immense. By taking each Organ, and considering its situation, figure, number, &c., he framed sixty-five Artificial Systems; and collected his Natural Families by a numerical combination of these. For example, his *sixty-fifth* Artificial System[25] is that which depends upon the situation of the Ovary with regard to the Flower; according to this system he frames *ten* Artificial Classes, including *ninety-three* Sections: and of these Sections the resulting Natural Arrangement retains *thirty-five*, above one-third: the same estimate is applied in other cases.

But this attempt to make Number supply the defects which the vague notion of Resemblance introduces, however ingenious, must end in failure. For, as Decandolle observes[26], it supposes that we know, not only all the Organs of plants, but all the points of view in which it is possible to consider them; and even if this assumption were true, which it is not, and must long be very far from being, the principle is altogether vicious; for it supposes that all these points of view, and all the resulting artificial systems are of equal importance:—a supposition manifestly erroneous. We are thus led back to the consideration of the *Relative Importance* of Organs and their qualities, as a basis for the classification of plants, which no Artificial Method can supersede; and thus we find the necessity of attending to something besides mere external and detached Resemblance. The method of General Comparison cannot, any more than the method of Blind Trial, lead us, with any certainty or clearness, to the Natural Method. Adanson's Families are held by the best botanists to be, for the greater part, Natural; but his hypotheses are unfounded; and his success is

[25] Adanson, Pref. p. cccxii.

[26] Dec. *Theor. Elem.* p. 67.

probably more due to the dim feeling of Affinity, by which he was unconsciously guided, than to the help he derived from his numerical processes.

In a succeeding chapter I shall treat of that Natural Affinity on which a Natural System must really be founded. But before proceeding to this higher subject, we must say a few words on some of the other parts of the philosophy of Natural History,—the Gradation of Groups, the Nomenclature, the Diagnosis, and the application of the methods to other subjects.

SECT. V.—*Gradation of Groups.*

15. It has been already noticed (last chapter,) that even that vague application of the idea of resemblance which gives rise to the terms of common language, introduces a subordination of classes, as *man, animal, body, substance.* Such a subordination appears in a more precise form when we employ this idea in a scientific manner as we do in Natural History. We have then a series of divisions, each inclusive of the lower ones, which are expressed by various metaphors in different writers. Thus some have gone as far as eight terms of the series[27], and have taken, for the most part, military names for them; as *Hosts, Legions, Phalanxes, Centuries, Cohorts, Sections, Genera, Species.* But the most received series is *Classes, Orders, Genera*, and *Species;* in which, however, we often have other terms interpolated, as *Sub-genera*, or Sections of genera. The expressions *Family* and *Tribe*, are commonly appropriated to natural groups; and we speak of the Vegetable, Animal, Mineral *Kingdom;* but the other metaphors of Provinces, Districts, &c., which this suggests, have not been commonly used[28].

It will of course be understood that each ascending step of classification is deduced by the same process from the one below. A Genus is a collection of Species which resemble each other more than they resem-

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[27] Adanson, p. cvi.

[28] *Sub-Kingdom* has recently been employed by some naturalists.

ble other species; an Order is a collection of Genera having, in like manner, the first degree of resemblance, and so on. How close or how wide the Degrees of Resemblance are, must depend upon the nature of the objects compared, and cannot possibly be prescribed beforehand. Hence the same term, *Class* and *Order* for instance, may imply, in different provinces of nature, very different degrees of resemblance. The Classes of Animals are Insects, Birds, Fish, Beasts, &c. The Orders of Beasts are *Ruminants*, *Tardigrades*, *Plantigrades*, &c. The two Classes of Plants (according to the Natural Order[29]) are *Vascular* and *Cellular*, the latter having neither sexes, flowers, nor spiral vessels. The Vascular Plants are divided into Orders, as *Umbelliferæ*, *Ranunculaceæ*, &c.; but between this Class and its Orders are interposed two other steps:—two Sub-classes, *Dicotyledonous* and *Monocotyledonous*, and two Tribes of each: *Angiospermiæ*, *Gymnospermiæ* of the first; and *Petaloideæ*, *Glumaciæ* of the second. Such interpolations are modifications of the general formula of subordination, for the purpose of accommodating it to the most prominent natural affinities.

16. *Species.*—As we have already seen in tracing the principles of the Natural Method, when by the intimate study of plants we seek to give fixity and definiteness to the notion of resemblance and affinity on which all these divisions depend, we are led to the study of Organization and Analogy. But we make a reference to physiological conditions even from the first, with regard to the lowest step of our arrangement, the *Species;* for we consider it a proof of the impropriety of separating two Species, if it be shown that they can by any course of propagation, culture, and treatment, the one pass into the other. It is in this way, for example, that it has been supposed to be established that the common Primrose, Oxlip, Polyanthus, and Cowslip, are all the same species. Plants which thus, in virtue of external circumstances, as soil,

[29] Lindley.

exposure, climate, exhibit differences which may disappear by changing the circumstances, are called *Varieties* of the species. And thus we cannot say that a Species is a collection of individuals which possess the First Degree of Resemblance; for it is clear that a primrose resembles another primrose more than it does a cowslip; but this resemblance only constitutes a Variety. And we find that we must necessarily include in our conception of Species, the notion of propagation from the same stock. And thus a Species has been well defined[30]: 'The collection of the individuals descended from one another, or from common parents, and of those which resemble these as much as these resemble each other.' And thus the sexual doctrine of plants, or rather the consideration of them as things which propagate their kind, (whether by seed, shoot, or in any other way,) is at the basis of our classifications.

17. The First permanent Degree of Resemblance among organized beings is thus that which depends on this relation of generation, and we might expect that the groups which are connected by this relation would derive their names from the notion of generation. It is curious that both in Greek and Latin languages and in our own, the words which have this origin (γένος, *genus, kind,*) do not, in the phraseology of science at least, denote the nearest degree of relationship, but have other terms subordinate to them, which appear etymologically to indicate a mere resemblance of appearance (εἶδος, *species, sort*); and these latter terms are appropriated to the groups resulting from propagation. Probably the reason of this is, that the former terms (*genus*, &c.) had been applied so widely and loosely before the scientific fixation of terms, that to confine them to what we call *species* would have been to restrict them in a manner too unusual to be convenient.

18. *Varieties. Races.*—The Species, as we have said, is the collection of individuals which resemble each other as much as do the offspring of a common

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[30] Cuv. *Règne Animal*, p. 19.

stock. But within the limits of this boundary, there are often observable differences permanent enough to attract our notice, though capable of being obliterated by mixture in the course of generation. Such different groups are called *Varieties.* Thus the Primrose and Cowslip, as has been stated above, are found to be varieties of the same plant; the Poodle and the Greyhound are well marked varieties of the species *dog.* Such differences are hereditary, and it may be long doubtful whether such hereditary differences are varieties only, or different species. In such cases the term *Race* has been applied.

Sect. VI.—*Nomenclature.*

19. The Nomenclature of any branch of Natural History is the collection of names of all its species; which, when they become extremely numerous, requires some artifice to make it possible to recollect or apply them. The known species of plants, for example, were 10,000 at the time of Linnæus, and are now probably 60,000. It would be useless to endeavour to frame and employ separate names for each of these species.

The division of the objects into a subordinated system of classification enables us to introduce a Nomenclature which does not require this enormous number of names. The artifice employed to avoid this inconvenience is to name a Species by means of two (or it might be more) steps of the successive division. Thus in Botany, each of the genera has its name, and the species are marked by the addition of some epithet to the name of the genus. In this manner about 1,700 generic names, with a moderate number of specific names, were found by Linnæus sufficient to designate with precision all the species of vegetables known at his time. And this *Binary Method* of Nomenclature has been found so convenient that it has been universally adopted in every other department of the Natural History of organized beings.

Many other modes of Nomenclature have been tried, but no other has at all taken root. Linnæus himself

appears at first to have intended marking each species by the Generic Name accompanied by a characteristic Descriptive Phrase; and to have proposed the employment of a *trivial* Specific Name, as he termed it, only as a method of occasional convenience. The use of these *trivial names*, has, however, become universal, as we have said, and is by many persons considered the greatest improvement introduced at the Linnæan reform.

Both Linnæus and other writers (as Adanson) have given many maxims with a view of regulating the selection of generic and specific names. The maxims of Linnæus were intended as much as possible to exclude barbarism and confusion, and have, upon the whole, been generally adopted; though many of them were objected to by his contemporaries (Adanson and others[31]), as capricious or unnecessary innovations. Many of the names, introduced by Linnæus, certainly appear fanciful enough: thus he gives the name of *Bauhinia* to a plant with leaves in pairs, because the Bauhins were a pair of brothers; *Banisteria* is the name of a climbing plant, in honour of Banister, who travelled among mountains. But such names, once established by adequate authority, lose all their inconvenience, and easily become permanent; and hence the reasonableness of the Linnæan rule[32], that as such a perpetuation of the names of persons by the names of plants is the only honour botanists have to bestow, it ought to be used with care and caution.

The generic name must, as Linnæus says, be fixed[33] before we attempt to form a specific name; 'the latter without the former is like the clapper without the bell.' The name of the genus being established, the species may be marked by adding to it 'a single word taken at will from any quarter;' that is, not involving a description or any essential property of the plant, but a casual or arbitrary appellation[34]. Thus the vari-

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[31] Pp. cxxix. clxxii.

[32] *Phil. Bot.* s. 239.

[33] *Ib.* s. 222.

[34] *Ib.* s. 260.

ous species of *Hieracium*[35] are *Hieracium Alpinum*, *H. Halleri*, *H. Pilosella*, *H. dubium*, *H. murorum*, &c. where we see how different may be the kind of origin of the words.

Attempts have been made at various times to form the name of species from those of genera in some more symmetrical manner. Thus some have numbered the species of genus, 1, 2, 3, &c.; but this method is liable to the inconveniences, first, that it offers nothing for the memory to take hold of; and second, that if a new species intermediate between 1 and 2, 2 and 3, &c., be discovered, it cannot be put in its place. It has also been proposed to mark the species by altering the termination of the genus. Thus Adanson[36], denoting a genus by the name *Fonna* (*Lychnidea*), conceived he might mark five of its species by altering the last vowel, *Fonna*, *Fonna-e*, *Fonna-i*, *Fonna-o*, *Fonna-u;* then others by *Fonna-ba*, *Fonna-ka*, and so on. This course would be liable to the same evils which have been noticed as belonging to the numerical method.

The names of plants (and the same is true of animals) have in common practice been binary only, consisting of a generic and a specific name. The Class and Order have not been admitted to form part of the appellation of the species. Indeed it is easy to see that a name which must be identical in so many instances as that of an Order would be, would be felt as superfluous and burdensome. Accordingly, Linnæus makes it a precept[37], that the name of the Class and the Order must not be expressed but understood: and hence, he says, Royen, who took *Lilium* for the name of a Class, rightly rejected it as a generic name, and substituted *Lirium*, with the Greek termination.

Yet we must not too peremptorily assume such maxims as these to be universal for all classificatory sciences. It is very possible that it may be found advisable to use *three* terms, that of order, genus and

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[35] Hooker, *Fl. Scot.* 228. [36] Pref. clxxvi. [37] *Phil. Bot.* s. 215.

species, in designating minerals, as is done in Mohs's nomenclature; for example, *Rhombohedral Calc Haloide, Paratomous Hal Baryte.*

It is possible also that it may be found useful in the same science to mark some of the steps of classification by the termination. Thus it has been proposed to confine the termination *ite* to the Order *Silicides* of Naumann, as Apophyll*ite*, Stilb*ite*, Leuc*ite*, &c., and to use names of different form in other orders, as Talc *Spar* for Brennerite, Pyramidal Titanium *Oxide* for Octahedrite. Some such method appears to be the most likely to give us a tolerable mineralogical nomenclature.

SECT. VII.—*Diagnosis.*

20. German Naturalists speak of a part of the general method which they call the *Characteristik* of Natural History, and which is distinguished from the *Systematik* of the science. The *Systematick* arranges the objects by means of all their resemblances, the *Characteristick* enables us to detect their place in the arrangement by means of a few of their characters. What these characters are to be, must be discovered by observation of the groups and divisions of the system when they are formed. To construct a collection of such characters as shall be clear and fixed, is a useful, and generally a difficult task; for there is usually no apparent connexion between the marks which are used in discriminating the groups, and the nature of the groups themselves. They are assumed only because the naturalist, extensively and exactly acquainted with the groups and the properties of the objects which compose them, sees, by a survey of the field, that these marks divide it properly.

The Characteristick has been termed by some English Botanists the *Diagnosis* of plants; a word which we may conveniently adopt. The Diagnosis of any genus or species is different according to the system we follow. Thus in the Linnæan System the Diagnosis of the Rose is in the first place given by its Class and Order: it is

Icosandrous, and Polygynous; and then the Generic Distinction is that the calyx is five-cleft, the tube urceolate, including many hairy achenia, the receptacle villous[38]. In the Natural System the Rose-Tribe are distinguished as being[39] 'Polypetalous dicotyledons, with lateral styles, superior simple ovaria, regular perigynous stamens, exalbuminous definite seeds, and alternate stipulate leaves.' And the true Roses are further distinguished by having 'Nuts, numerous, hairy, terminated by the persistent lateral style and inclosed within the fleshy tube of the calyx,' &c.

It will be observed that in a rigorous Artificial System the *Systematick* coincides with the *Characteristick;* the *Diataxis* with the *Diagnosis;* the reason why a plant is put in a division is identical with the mode by which it is known to be in the division. The Rose is in the class *icosandria*, because it has many stamens inserted in the calyx; and when we see such a set of stamens we immediately know the class. But this is not the case with the Diagnosis of Natural Families. Thus the genera *Lamium* and *Galeopsis* (Dead Nettle and Hemp Nettle) are each formed into a separate group in virtue of their general resemblances and differences, and not because the former has one tooth on each side of the lower lip, and the latter a notch in its upper lip, though they are distinguished by these marks.

Thus so far as our Systems are natural, (which, as we have shown, all systems to a certain extent must be), the Characteristick is distinct both from a Natural and an Artificial System; and is, in fact, an Artificial Key to a Natural System. As being Artificial, it takes as few characters as possible; as being Natural, its characters are not selected by any general or prescribed rule, but follow the natural affinities. The Botanists who have made any steps in the formation of a natural method of plants since Linnæus, have all attempted to give a Diagnosis corresponding to the Diataxis of their method.

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38 Lindley, *Nat. Syst.* p. 149.

39 *Ib.* pp. 81, 3.

CHAPTER III.

Application of the Natural History Method to Mineralogy.

1. THE philosophy of the Sciences of Classification has had great light thrown upon it by discussions concerning the methods which are used in Botany: for that science is one of the most complete examples which can be conceived of the consistent and successful application of the principles and ideas of Classification; and this application has been made in general without giving rise to any very startling paradoxes, or disclosing any insurmountable difficulties. But the discussions concerning methods of Mineralogical Classification have been instructive for quite a different reason: they have brought into view the boundaries and the difficulties of the process of Classification; and have presented examples in which every possible mode of classifying appeared to involve inextricable contradictions. I will notice some of the points of this kind which demand our attention, referring to the works published recently by several mineralogists.

In the History of Mineralogy we noticed the attempt made by Mohs and other Germans to apply to minerals a method of arrangement similar to that which has been so successfully employed for plants. The survey which we have now taken of the grounds of that method will point out some of the reasons of the very imperfect success of this attempt. We have already said that the *Terminology* of Mineralogy was materially reformed by Werner; and including in this branch of the subject (as we must do) the Crystallography of later writers, it may be considered as to a great extent complete. Of the attempts at a Natural arrangement, that of Mohs appears to proceed by the

method of *blind trial*, the undefinable perception of relationship, by which the earliest attempts at a Natural Arrangement of plants were made. Breithaupt however, has made (though I do not know that he has published) an essay in a mode which corresponds very nearly to Adanson's process of *multiplied comparisons.* Having ascertained the specific gravity and hardness of all the species of minerals, he arranged them in a table, representing by two lines at right angles to each other these two numerical quantities. Thus all minerals were distributed according to two co-ordinates representing specific gravity and hardness. He conceived that the groups which were thus brought together were natural groups. On both these methods, and on all similar ones, we might observe, that in minerals as in plants, the mere general notion of Likeness cannot lead us to a real arrangement : this notion requires to have precision and aim given it by some other relation ;—by the relation of Chemical Composition in minerals, as by the relation of Organic Function in vegetables. The physical and crystallographical properties of minerals must be studied with reference to their constitution ; and they must be arranged into Groups which have some common Chemical Character, before we can consider any advance as made towards a Natural Arrangement.

In reality, it happens in Mineralogy as it happened in Botany, that those speculators are regulated by an obscure perception of this ulterior relation, who do not profess to be regulated by it. Several of the Orders of Mohs have really great unity of chemical character, and thus have good evidence of their being really Natural Orders.

2. Supposing the Diataxis of minerals thus obtained, Mohs attempted the Diagnosis ; and his *Characteristick of the Mineral Kingdom*, published in Dresden, in 1820, was the first public indication of his having constructed a system. From the nature of a Characteristick, it is necessarily brief, and without any ostensible principle ; but its importance was duly appreciated by the author's countrymen. Since that

time, many attempts have been made at improved arrangements of minerals, but none, I think, (except perhaps that of Breithaupt,) professing to proceed rigorously on the principles of Natural History;—to arrange by means of external characters, neglecting altogether, or rather postponing, the consideration of chemical properties. By relaxing from this rigour, however, and by combining physical and chemical considerations, arrangements have been obtained (for example, that of Naumann,) which appear more likely than the one of Mohs to be approximations to an ultimate really natural system. Naumann's Classes are *Hydrolytes*, *Haloides*, *Silicides*, *Metal Oxides*, *Metals*, *Sulphurides*, *Anthracides*, with subdivisions of Orders, as *Anhydrous unmetallic Silicides*. It may be remarked that the designations of these are mostly chemical. As we have observed already, Chemistry, and Mineralogy in its largest sense, are each the necessary supplement of the other. If Chemistry furnish the Nomenclature, Mineralogy must supply the Physiography: if the Arrangement be founded on External Characters and the Names be independent of Chemistry, the chemical composition of each species is an important scientific Truth respecting it.

3. The inquiry may actually occur, whether any subordination of groups in the mineral kingdom has really been made out. The ancient chemical arrangements, for instance, that of Haüy, though professing to distribute minerals according to Classes, Orders, Genera, and Species, were not only arbitrary, but inapplicable; for the first postulate of any method, that the species should have constant characters of unity and difference, was not satisfied. It was not ascertained that carbonate of lime was really distinguishable in all cases from carbonate of magnesia, or of iron; yet these species were placed in remote parts of the system: and the above carbonates made just so many species; although, if they were distinct from one another at all, they were further distinguishable into additional species. Even now, we may, perhaps, say that the limits of mineralogical species, and their laws of fixity, are

not yet clearly seen. For the discoveries of the isomorphous relations and of the optical properties of minerals have rather shown us in what direction the object lies, than led us to the goal. It is clear that, in the mineral kingdom, the Definition of Species, borrowed from the laws of the continuation of the kind, which holds throughout the organic world, fails us altogether, and must be replaced by some other condition: nor is it difficult to see that the definite atomic relations of the chemical constituents, and the definite crystalline angle, must supply the principles of the *Specific* Identity for minerals. Yet the exact limits of definiteness in both these cases (when we admit the effect of mechanical mixtures, &c.) have not yet been completely disentangled. Moreover, any *arbitrary* assumption (as the allowance of a certain per-centage of mixture, or a certain small deviation in the angle,) is altogether contrary to the philosophy of the Natural System, and can lead to no stable views. It is only by laborious, extensive, and minute research, that we can hope to attain to any solid basis of arrangement.

4. Still, though there are many doubts respecting mineralogical species, a large number of such species are so far fixed that they may be supposed capable of being united under the higher divisions of a system with approximate truth. Of these higher divisions, those which have been termed *Orders* appear to tend to something like a fixed chemical character. Thus the *Haloids* of Naumann, and mostly those of Mohs, are combinations of an oxide with an acid, and thus resemble Salts, whence their name. The Silicides contain most of Mohs's *Spaths:* and the Orders *Pyrites, Glance,* and *Blende,* are common to Naumann and Mohs; being established by the latter on a difference of external character, which difference is, indeed, very manifest; and being included by the former in one chemical *Class, Sulphurides.* The distinctions of *Hydrous* and *Anhydrous, Metallic* and *Unmetallic,* are, of course, chemical distinctions, but occur as the differences of Orders in Naumann's mixed system.

We may observe that some French writers, following Haüy's last edition, use, instead of *metallic* and *unmetallic*, *autopside metallic* and *heteropside metallic*; meaning by this phraseology to acknowledge the discovery that earths, &c., *are* metallic, though they do not *appear* to be so, while metals both are and appear metallic. But this seems to be a refinement not only useless but absurd. For what is gained by adding the word *metallic*, which is common to all, and therefore makes no distinction? If certain metals are distinguished by their *appearing* to be metals, this appearance is a reason for giving them the peculiar name, *metals*. Nothing is gained by first bringing earths and metals together, and then immediately separating them again by new and inconvenient names. No proposition can be expressed better by calling *earths, heteropside metallic substances*, and therefore such nomenclature is to be rejected.

Granting, then, that the Orders of the best recent mineralogical systems approximate to natural groups, we are led to ask whether the same can be said of the Genera of the Natural History systems, such as those of Mohs and Breithaupt. And here I must confess that I see no principle in these Genera; I have failed to apprehend the conceptions by the application of which they have been constructed: I shall therefore not pass any further judgment upon them. The subordination of Mineralogical Species to Orders is a manifest gain to science: in the interposition of Genera I see nothing but a source of confusion.

5. In Mineralogy, as in other branches of natural history, a reformed arrangement ought to give rise to a reformed Nomenclature; and for this, there is more occasion at present in Mineralogy than there was in Botany at the worst period, at least as far as the extent of the subject allows. The characters of minerals are much more dimly and unfrequently developed than those of plants; hence arbitrary chemical arrangements, which could not lead to any natural groups, and therefore not to any good names, prevailed till recently; and this state of things produced an anarchy

in which every man did what seemed right in his own eyes,—proposed species without any ascertained distinction, and without a thought of subordination, and gave them arbitrary names; and thus with only about two or three hundred known species, we have thousands upon thousands of names, of anomalous form and uncertain application.

Mohs has attempted to reform the Nomenclature of the subject in a mode consistent with his attempt to reform the System. In doing this, he has fatally transgressed a rule always insisted upon by the legislators of Botany, of altering usual names as little as possible; and his names are both so novel and so cumbrous, that they appear to have little chance of permanent currency. They are, perhaps, more unwieldy than they need to be, by referring, as we have said, to *three* of the steps of his classification, the Species, Genus, and Order. We may, however, assert confidently, from the whole analogy of natural history, that no good names can be found which do not refer to at least *two* terms of the arrangement. This rule has been practically adopted to a great extent by Naumann, who gives to most of his Haloids the name *Spar*, as Calc spar, Iron spar, &c.; to all his Oxides the terminal word *Erz* (*Ore*); and to the species of the orders *Kies* (*Pyrites*), *Glance*, and *Blende*, these names. It has also been theoretically assented to by Beudant, who proposes that we should say *silicate stilbite*, *silicate chabasie; carbonate calcaire, carbonate witherite; sulphate couperose*, &c. One great difficulty in this case would arise from the great number of *silicides;* it is not likely that any names would obtain a footing which tacked the term *silicide* to another word for each of these species. The artifice which I have proposed, in order to obviate this difficulty, is that we should make the names of the silicides, and those alone, end in *ite* or *lite*, which a large proportion of them do already.

By this and a few similar contrivances, we might, I conceive, without any inconvenient change, introduce into Mineralogy a systematic nomenclature.

6. I shall now proceed to make a few remarks on a work on Mineralogy more recent than those which I have above noticed, and written with express reference to such difficulties as I have been discussing. I allude to the treatise of M. Necker, *Le Règne Mineral ramené aux Methodes d'Histoire Naturelle*[1], which also contains various dissertations on the Philosophy of Classification in general, and its application to Mineralogy in particular.

M. Necker remarks very justly, that Mineralogy, as it has hitherto been treated, differs from all other branches of Natural History in this:—that while it is invested with all the forms of the sciences of classification,—Classes, Divisions, Genera, and the like,—the properties of those bodies to which the mineralogical student's attention is directed have no bearing whatever on the classification. A person, he remarks[2], might be perfectly well acquainted with all the characters of minerals which Werner or Haüy examined so carefully, and might yet be quite unable to assign to any mineral its place in the divisions of their methods. There is[3] a complete separation between the study of mineralogical characters and the recognition of the name and systematic place of a mineral. Those who know *mineralogy* well, may know *minerals* ill, or hardly at all; the systematist may be in such knowledge vastly inferior to the mineral-dealer or the miner. In this respect there is a complete contrast between this science and other classificatory sciences.

Again, in the best-known systems of Mineralogy, (as those of Werner and Haüy,) the bodies which are grouped together as belonging to the same division, have not, as they have in other classificatory sciences, any resemblance. The different members of the larger classes are united by the common possession of some abstract property,—as, that they all contain iron. This is a property to which no common circumstance in the bodies themselves corresponds. What is there common to the minerals named oxidulous iron, sulphuret

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1 Paris, 1835. 2 *Règne Mineral*, p. 3. 3 *Ib.* p. 8.

of iron, carbonate of iron, sulphate of iron, except that they all contain iron? And when we have classed these bodies together, what general assertion can we make concerning them, except that which is the ground of our classification, that they contain iron? They have nothing in common with iron or with each other in any other way.

Again, as these classes have no general properties, all the properties are particular to the species; and the descriptions of these necessarily become both tediously long, and inconveniently insulated.

7. These inconveniences arise from making Chemical Composition the basis of Mineralogical Classification without giving Chemical Analysis the first place among Mineral Properties. Shall we, then, correct this omission, so far as it has affected mineralogical systems? Shall we teach the student the chemical analysis of minerals, and then direct him to classify them according to the results of his analysis[4]?

But why should we do this? To what purpose, or on what ground, do we arrange the results of chemical analysis according to the forms and subordination of natural history? Is not Chemistry a science distinct from Natural History? Are not the sciences opposed? Is not natural history confined to organic bodies? Can mere chemical elements and their combinations be, with any propriety or consistency, arranged into Species, Genera, and Families? What is the principle on which genera and species depend? Do not Species imply Individuals? What is an Individual in the case of a chemical substance?

8. We thus find some of the widest and deepest questions of the philosophy of classification brought under our consideration when we would provide a method for the classification of minerals. The answers to these questions are given by M. Necker; and I shall state some of his opinions; taking the liberty of adding such remarks as are suggested by referring the subject

[4] *Règne Mineral*, p. 18.

to those principles which have already been established in this work.

M. Necker asserts[5] that the distinctions of different Sciences depend, not on the objects they consider, but on the different and independent points of view on which they proceed. Each science has its logic, that is, its mode of applying the general rules of human reason to its own special case. It has been said by some[6], that in minerals, natural history and chemistry contemplate common objects, and thus form a single science. But do chemistry and natural history consider minerals in the same point of view?

The answer is, that they do not. Physics and Chemistry consider the properties of bodies in an abstract manner; as, their composition, their elements, their mutual actions, with the laws of these; their forces, as attraction, affinity; all which objects are abstract ideas. In these cases we have nothing to do with bodies themselves, but as the vehicles of the powers and properties which we contemplate.

Natural History, on the other hand, has to do with natural bodies: their properties are not considered abstractedly, but only as characters. If the properties are abstracted, it is but for a moment. Natural history has to describe and class bodies as they are. All which cannot be perceived by the senses, belongs not to its domain, as molecules, atoms, elements.

Natural history[7] may have recourse to physics or chemistry in order to recognize those properties of bodies which serve as characters; but natural history is not, on that account, physics or chemistry. Classification is the essential business of the natural historian[8], to which task chemistry and physics are only instrumental, and the further account of properties only complementary.

It has been said, in support of the doctrine that chemistry and mineralogy are identical, that chemistry does not neglect external characters. 'The chemist in

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[5] *Règne Mineral*, p. 23.

[6] *Ib.* p. 27.

[7] *Ib.* p. 37.

[8] *Ib.* p. 41.

describing sulphur, mentions its colour, taste, odour, hardness, transparence, crystalline form, specific gravity; how does he then differ from the mineralogist?' But to this it is replied, that these notices of the external characters of this or any substance are introduced in chemistry merely as convenient marks of recognition; whereas they are essential in mineralogy. If we had taken the account given of several substances instead of one, we should have seen that the chemist and the naturalist consider them in ways altogether different. The chemist will make it his business to discover the mutual action of the substances; he will combine them, form new products, determine the proportions of the elements. The mineralogist will divide the substances into groups according to their properties, and then subdivide these groups, till he refers each substance to its species. Exterior and physical characters are merely accessory and subordinate for the chemist; chemistry is merely instrumental for the mineralogist.

This view agrees with that to which we have been led by our previous reasonings; and may, according to our principles, be expressed briefly by saying, that the Idea which Chemistry has to apply is the Idea of Elementary Composition, while Natural History applies the Idea of Graduated Resemblances, and thus performs the task of classification.

9. The question occurs[9], whether Natural History can be applied to Inorganic Substances? And the answer to this question is, that it can be applied, if there are such things as inorganic *individuals*, since the resemblances and differences with which natural history has to do are the resemblances and differences of individuals.

What is an Individual? It certainly is not that which is so simple that it cannot be divided. Individual animals are composed of many parts. But if we examine, we shall find that our Idea of an Individual is, that it is a whole composed of parts, which

[9] *Règne Minéral*, p. 46.

are not similar to the whole, and have not an independent existence, while the whole has an independent existence and a definite form[10].

What then is the Mineralogical Individual? At first, while minerals were studied for their use, the most precious of the substances which they contained was looked upon as the characteristic of the mineral. The smallest trace of silver made a mineral an *ore of silver*. Thus forms and properties were disregarded, and *substance* was considered as identical with *mineral*. And hence[11] Daubenton refused to recognize *species* in the mineral kingdom, because he recognized no individuals. He proposed to call *sorts* what we call species. In this way of considering minerals, there are no individuals.

10. But still this is not satisfactory: for if we take a well-formed and distinct crystal, this clearly *is* an individual[12].

It may be objected, that the crystal is divisible (according to the theory of crystallography) into smaller solids; that these small solids are really the simple objects; and that actual crystals are formed by combinations of these molecules according to certain laws.

But, as we have already said, an individual is such, not because it cannot be divided, but because it cannot be divided into parts similar to the whole. As to the division of the form into its component *laws*, this is an abstract proceeding, foreign to natural history[13]. Therefore there is so far nothing to prevent a crystal from being an individual.

11. We cannot (M. Necker goes on to remark) consider the *Integrant Molecules* as individuals. These are useful abstractions, but abstractions only, which we must not deal with as real objects. Haüy himself warns us[14] that his doctrine of increments is a purely abstract conception, and that nature, in fact, follows a different process. Accordingly, Weiss and Mohs express laws identical with those of Haüy, without even

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10 *Règne Mineral*, p. 52. 11 *Ib.* p. 54.
12 *Ib.* p. 56. 13 *Ib.* p. 58. 14 *Ib.* p. 61.

speaking of molecules; and Wollaston and Davy have deemed it probable that the molecules are not polyhedrons, but spheres or spheroids. Such mere creations of the mind can never be treated as individuals. If the maxim of natural history,—that the Species is a collection of Individuals—be applied so as to make those individuals mere abstractions; or if, instead of Individuals, we take such an abstraction as Substance or Matter, the course of natural history is altogether violated. And yet this errour has hitherto generally prevailed; and mineralogists have classified, not things, but abstract ideas[15].

12. But it may be said[16], will not the small solids obtained by Cleavage better answer the idea of individuals? To this it is replied, that these small solids have no independent existence. They are only the result of a mode of division. They are never found separate and independent. The secondary forms which they compose are determined by various circumstances (the nature of the solution, &c.); and the cleavage which produces these small solids is only one result among many, from the crystalline forces[17].

Thus neither Integrant Molecules, nor Solids obtained by Cleavage, can be such mineralogical Individuals as the spirit of natural history requires. Hence it appears that we must take the real Crystals for Individuals[18].

13. We must, however, reject crystals (generally large ones) which are obviously formed of several smaller ones of a similar form (as occurs so often in quartz and calc spar). We must also distinguish cases in which a large regular form is composed of smaller but different regular forms (as octahedrons of fluor spar made up of cubes). Here the small component forms are the individuals. Also we must notice the cases[19] in which we have a natural crystal, similar to the primary form. Here the face will show whether

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[15] *Règne Mineral*, p. 67. [16] *Ib.* p. 69.
[17] *Ib.* p. 71. [18] *Ib.* p. 73. [19] *Ib.* p. 75.

the body is a result obtained by cleavage or a natural individual.

14. It will be objected[20], that the crystalline form ought not to be made the dominant character in mineralogy, since it rarely occurs perfect. To this it is replied, that even if the application of the principle be difficult, still it has been shown to be the only true principle, and therefore we have no alternative. But further[21], it is not true that amorphous substances are more numerous than crystals. In Leonhard's *Manual of Oryctognosy*, there are 377 mineral substances. Of these, 281 have a crystalline structure, and 96 only have not been found in a regular form.

Again, the 281 crystalline forms have each its varieties, some of which are crystalline, and some are not so. Now the crystalline varieties amount to 1453, and the uncrystalline to 186 only. Thus mineralogy, according to the view of it here presented, has a sufficiently wide field[22].

15. It will be objected[23], that according to this mode of proceeding, we must reject from our system all non-crystalline minerals. But we reply, that if the mass be composed of crystals, the size of the crystals makes no difference. Now lamellar and other compact masses are very generally groups of crystals in various positions. Individuals mutilated and mixed together are not the less individuals; and therefore such masses may be treated as objects of natural history.

If we cannot refer all rocks to crystalline species, those which elude our method may appear as an appendix, corresponding to those plants which botanists call *genera incertæ sedis*[24].

But these genera and species will often be afterwards removed into the crystalline part of the system, by being identified with crystalline species. Thus *pyrope*, &c., have been referred to *garnet*, and *basalt*,

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[20] *Règne Minéral*, p. 79. [21] *Ib.* p. 82. [22] *Ib.* p. 84.
[23] *Ib.* p. 86. [24] *Ib.* p. 91.

wacke, &c., to compound rocks. Thus veins of *Dolerite*, visibly composed of two or three elements, pass to an apparently simple state by becoming fine-grained[25].

16. Finally[26], we have to ask, are artificial crystals to enter into our classification? M. Necker answers, No; because they are the result of art, like mules, mestizos, hybrids, and the like.

17. Upon these opinions, we may observe, that they appear to be, in the main, consistent with the soundest philosophy. That each natural crystal is an individual, is a doctrine which is the only basis of Mineralogy as a Natural Historical Science; yet the imperfections and confused unions of crystals make this principle difficult to apply. Perhaps it may be expressed in a more precise manner by referring to the crystalline forces, and to the axes by which their operation is determined, rather than to the external form. *That* portion of a mineral substance is a mineralogical *individual* which is determined by crystalline forces acting to the *same axes*. In this way we avoid the difficulty arising from the absence of faces, and enable ourselves to use either cleavage, or optical properties, or any others, as indications of the identity of the individual. The individual extends so far as the polar forces extend by which crystalline form is determined, whether or not those forces produce their full effect, namely, a perfectly circumscribed polyhedron.

18. There is only one material point on which our principles lead us to differ from M. Necker;—the propriety of including *artificial crystals* in our mineralogical classification. To exclude them, as he does, is a conclusion so entirely at variance with the whole course of his own reasonings, that it is difficult to conceive that he would persist in his conclusion, if his attention were drawn to the question more steadily. For, as he justly says[27], each science has its appropriate domain, determined by its peculiar point of view. Now artificial and natural crystals are considered in the same point of view, (namely, with reference to

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[25] *Règne Minéral*, p. 93. [26] *Ib.* p. 95. [27] *Ib.* p. 23.

crystalline, physical, and optical properties, as subservient to classification,) and ought, therefore, to belong to the same science. Again, he says[28], that Chemistry would reject as useless all notice of the physical properties and external characters of substances, if a *special science* were to take charge of the description and classification of these products. But such a special science must be Mineralogy; for we cannot well make one science of the classification of natural, and another of that of artificial substances: or if we do, the two sciences will be identical in method and principles, and will extend over each other's boundaries, so that it will be neither useful nor possible to distinguish them. Again, M. Necker's own reasonings on the selection of the individual in mineralogy are supported by well chosen examples[29]; but these examples are taken from artificial salts; as, for instance, common salt crystallizing in different mixtures. Again, the analogy of mules and mestizos, as products of art, with chemical compounds, is not just. Chemical compounds correspond rather to natural species, propagated by man under the most natural circumstances, in order that he may study the laws of their production[30].

19. But the decisive argument against the separation of natural and artificial crystals in our schemes of classification is, that we *cannot* make such a separation. Substances which were long known only as the products of the laboratory, are often discovered, after a time, in natural deposits. Are the crystals which are found in a forgotten retort or solution to be considered as belonging to a different science from those which occur in a deserted mine? And are the crystals which are produced where man has turned a stream of water or air out of its course, to be separated from natural crystals, when the composition, growth, and properties, are exactly the same in both? And again: How many natural crystals can we already produce by

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[28] *Règne Minéral*, p. 36. [29] *Ib.* p. 71.

[30] We may remark that M. Necker, in his own arrangement of minerals, inserts among his species Iron and Lead, which do not occur Native.

synthesis! How many more may we hope to imitate hereafter! M. Necker himself states[31], that Mitscherlich found, in the scoriæ of the mines of Sweden and Germany, artificial minerals having the same composition and the same crystalline form with natural minerals: as silicates of iron, lime, and magnesia, agreeing with Peridot; bisilicate of iron, lime, and magnesia, agreeing with Pyroxene; red oxide of copper; oxide of zinc; protoxide of iron (*fer oxydulé*); sulphurets of iron, zinc, lead; arseniuret of nickel; black mica. These were accidental results of fusion. But M. Berthier, by bringing together the elements in proper quantities, has succeeded in composing similar minerals, and has thus obtained artificial silicates, with the same forms and the same characters as natural silicates. Other chemists (M. Haldat, M. Becquerel) have, in like manner, obtained, by artificial processes, other crystals, known previously as occurring naturally. How are these crystals, thus identical with natural minerals, to be removed out of the domain of mineralogy, and transferred to a science which shall classify artificial crystals only? If this be done, the mineralogist will not be able to classify any specimen till he has human testimony whether it was found naturally occurring or produced by chemical art. Or is the other alternative to be taken, and are these crystals to be given up to mineralogy because they occur naturally also? But what can be more unphilosophical than to refer to separate sciences the results of chemical processes closely allied, and all but identical? The chemist constructs bisilicates, and these are classified by the mineralogist: but if he constructs a trisilicate, it belongs to another science. All these intolerable incongruities are avoided by acknowledging that artificial, as well as natural, crystals belong to the domain of mineralogy. It is, in fact, the *name* only of *Mineralogy* which appears to discover any inconsistency in this mode of proceeding. Mineralogy is the represen-

[31] *Règne Mineral*, p. 151.

tative of a science which has a wider office than mineralogists first contemplated; but which must exist, in order that the body of science may be complete. There must, as we have already said, be a Science, the object of which is to classify bodies by their physical characters, in order that we may have some means of asserting chemical truths concerning bodies; some language in which we may express the propositions which chemical analysis discovers. And this Science will have its object prescribed, not by any accidental or arbitrary difference of the story belonging to each specimen;—not by knowing whether the specimen was found in the mine or in the laboratory; produced by attempting to imitate nature, or to do violence to her:—but will have its course determined by its own character. The range and boundaries of this Science will be regulated by the Ideas with which it deals. Like all other sciences, it must extend to everything to which its principles apply. The limits of the province which it includes are fixed by the consideration that it must be a connected whole. No previous definition, no historical accident, no casual phrase, can at all stand in the way of philosophical consistency;—can make this Science exclude what that includes, or oblige it to admit what that rejects. And thus, whatever we call our Science;—whether we term it External Chemistry, Mineralogy, the Natural History of Inorganic Bodies;—since it can be nothing but the Science of the Classification of Inorganic Bodies of definite forms and properties, it must classify all such bodies, whether or not they be minerals, and whether or not they be natural.

20. In the application of the principles of classification to minerals, the question occurs, What are to be considered as mineral *Species?* By Species we are to understand, according to the usage of other parts of natural history, the lowest step of our subordinate divisions;—the most limited of the groups which have definite distinctions. What definite distinctions of groups of objects of any kind really occur in nature, is to be learnt from an examination of nature: and the

result of our inquiries will be some general principle which connects the members of each group, and distinguishes the members of groups which, though contiguous, are different. In the classification of organized bodies, the rule which thus presides over the formation of Species is the principle of *reproduction.* Those animals and those plants are of the same Species which are produced from a common stock, or which resemble each other as much as the progeny of a common stock. Accordingly in practice, if any questions arise whether two varieties of form in organic things be of the same or different species, it is settled by reference to the fact of reproduction; and when it is ascertained that the two forms come within the habitual and regular limits of a common circle of reproduction, they are held to be of the same species. Now in crystals, this principle of reproduction disappears altogether, and the basis of the formation of species must be sought elsewhere. We must have some other principle to replace the reproduction which belongs only to organic life. This principle will be, we may expect, one which secures the permanence and regularity of mineral forms, as the reproductive power does of animal and vegetable. Such a principle is the *Power of Crystallization.* The forces of which solidity, cohesion, and crystallization are the result, are those which give to minerals their permanent existence and their physical properties; and ever since the discovery of the distinctions of Crystalline Forms and Crystalline Systems, it is certain that this force distinguishes groups of crystals in the most precise and definite manner. The rhombohedral carbonates of lime and of iron, for instance, are distinguished exactly by the angles of their rhombohedrons. And if, in the case of any proposed crystal, we should doubt to which kind the specimen belongs, the measurement of the angles of cleavage would at once decide the question. The principle of Crystallization therefore appears, from analogy, to be exactly fitted to take the place of the principle of organic Generation. The forces which make the individual permanent and its properties definite, here stand in the place of the forces

which preserve the race, while individuals are generated and die.

21. According to this view, the different Modifications of the *same* crystalline form would be *Varieties* only of the same Species. All the various solids, for example, which are produced by the different laws of derivation of rhombohedral carbonate of lime, would fall within the same Species. And this appears to be required by the general analogy of Natural History. For these differences of form, produced by the laws of crystalline derivation, are not *definite.* The faces which are added to one form in order to produce another, may be of any size, small or large, and thus the crystal which represents one modification passes by insensible degrees to another. The forms of calc spar, which we call *dog-tooth spar*, *cannon spar*, *nail-head spar*, and the like, appear at first, no doubt, distinct enough; but so do the races of dogs. And we find, in the mineral as in the animal, that the distinction is obliterated by taking such intermediate steps as really occur. And if a *fragment* of any of these crystals is given us, we can determine that it is rhombohedral carbonate of lime; but it is not possible, in general, to determine to which of the kinds of crystals it has belonged.

22. Notwithstanding these considerations, M. Necker has taken for his basis of mineral species[32] the *Secondary* Modifications, and not the Primary Forms. Thus *cubical galena*, *octahedral galena*, and *triform galena*, are, with him, three *species* of crystals.

On this I have to observe, as I have already done, that on this principle we have no *definite* distinction of species; for these forms may and do pass into each other: among cubo-octahedrons of galena occur cubes and octahedrons, as one face or another vanishes, and the transition is insensible. We shall, on this principle, find almost always three or four species in the same tuft of crystals; for almost every individual in such assemblages may exhibit a different combination of

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[32] *Règne Mineral*, p. 396.

secondary faces. Again, in cases where the secondary laws are numerous, it would be impracticable to enumerate all their combinations, and impossible therefore to give a list of species. Accordingly M. Necker[33] gives seventy-one Species of *spath calcaire*, and then says, 'Nous n'avons pas énuméré la dixième partie des espèces connues de ce genre, qui se montent à plus de huit cents.' Again, in many substances, of which few crystals are found, every new specimen would be a new species; if indeed it were perfect enough to be referred to a species at all. But from a specimen without perfect external form, however perfect in crystalline character, although everything else might be known,—angles, optical properties, physical properties, and chemical constitution,—the species could not be determined. Thus M. Necker says[34] of the micas, 'Quant aux espèces propre à chaque genre, la lacune sera presque complète; car jusqu'ici les cristaux entiers de Mica et de Talc n'ont pas été fort communs.'

These inconveniences arise from neglecting the leading rule of natural history, that the *predominant principle* of the existence of an object must determine the Species; whether this principle be Reproduction operating for Development, or Crystallization operating for Permanence of form. We may add to the above statement of inconveniences this;—that if M. Necker's view of mineralogical species be adopted, the distinction of Species is vague and indefinite, while that of Genera is perfectly precise and rigorous;—an aspect of the system entirely at variance with other parts of Natural History; for in all these the Species is a more definite group than the Genus.

This result follows, as has already been said, from M. Necker's wish to have individuals marked by external form. If, instead of this, we are contented to take for an individual that portion of a mass, of whatever form, which is connected by the continuous influence of the same crystalline forces, by whatever incidents these forces may be manifested, (as cleavage,

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[33] *Règne Minéral*, p. 364.

[34] *Ib.* ii. 414.

physical and optical properties, and the like,) our mode of proceeding avoids all the above inconveniences, applies alike to the most perfect and most imperfect specimens, and gives a result agreeable to the general analogy of natural history, and the rules of its methods[35].

I now quit the subject of mere Resemblance, and proceed to treat of that natural affinity which Natural Systems of Classification for organic bodies must involve.

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[35] I will not again enter into the subject of Nomenclature; but I may remark that M. Necker has adopted (i. 415) the Nomenclature of Beudant, latinizing the names, and thus converting each into a single word. He has also introduced, besides the names of Genera, names of Families taken from the *typical* Genus. Thus the Family of *Carbonidiens* contains the following genera: *Calcispathum*, *Magnesispathum*, *Dolomispathvm*, *Ferrispathum*, &c., *Malachita*, *Azuria*, *Gaylusacia*.

CHAPTER IV.

Of the Idea of Natural Affinity.

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1. IN the Second Chapter of this Book it was shown that although the Classificatory Sciences proceed ostensibly upon the Idea of Resemblance as their main foundation, they necessarily take for granted in the course of their progress a further Idea of Natural Affinity. This appeared[1] by a general consideration of the nature of Science, by the recognition of natural species and genera, even in Artificial Systems of Classification[2], and by the attemps of botanists to form a Natural System. It further appeared that among the processes by which endeavours have been made to frame a Natural System, some, as the method of *Blind Trial* and the method of *General Comparison*, have been altogether unsuccessful, being founded only upon a collection of resemblances, casual in the one case and arbitrary in the other. In neither of these processes is there employed any general principle by which we may be definitely directed as to what resemblances we should employ, or by which the result at which we arrive may be verified and confirmed. Our object in the present chapter is to show that the Idea of Natural Affinity supplies us with a principle which may answer such purposes.

I shall first consider the Idea of Affinity as exemplified in organized beings. In doing this, we may appear to take for granted Ideas which have not yet come under our discussion, as the Ideas of Organization, and Vital Function; but it will be found that the principle to which we are led is independent of these additional Ideas.

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[1] Art. 5.

[2] Art. 7.

2. We have already seen that the attempts to discover the divisions which result from this Natural Affinity have led to the consideration of the *Subordination of Characters.* It is easy to see that some organs are more essential than others to the existence of an organized being; the organs of nutrition, for example, more essential than those of locomotion. But at the same time it is clear that any *arbitrary* assumption of a certain scale of relative values of different kinds of characters will lead only to an Artificial System. This will happen, if, for example, we begin by declaring the nutritive to be superior in importance to the reproductive functions. It is clear that this relation of importance of organs and functions must be collected by the study of the organized beings; and cannot be determined *à priori,* without depriving us of all right to expect a general accordance between our system and the arrangement of nature. We see, therefore, that our notion of Natural Affinity involves in it this consequence;—that it is not to be made out by an *arbitrary* subordination of characters.

3. The functions and actions of living things which we separate from each other in our consideration, cannot be severed in nature. Each function is essential; Life implies a collection of movements, and ceases when any of these movements is stopped. A change in the organization subservient to one set of functions may lead necessarily to a change in the organization belonging to others. We can often see this necessary connexion; and from a comparison of the forms of organized beings,—from the way in which their structure changes in passing from one class to another, we are led to the conviction that there is some general principle which connects and graduates all such changes. When the circulatory system changes, the nervous system changes also: when the mode of locomotion changes, the respiration is also modified.

4. These corresponding changes may be considered as ways in which the living thing is fitted to its mode of life; as marks of *adaptation to a purpose;* or, as it has been otherwise expressed, as results of the *condi-*

tions of existence. But at the present moment, we put forward these correspondencies in a different light. We adduce them as illustrations of what we mean by Affinity, and what we consider as the tendency of a Natural Classification. It has sometimes been asserted that if we were to classify any of the departments of organized nature by means of one function, and then by means of another, the two classifications, if each strictly consistent with itself, would be consistent with each other. Such an assertion is perhaps more than we are entitled to make with confidence; but it shows very well what is meant by Affinity. The disposition to believe such a general identity of all partial natural classifications, shows how readily we fix upon the notion of Affinity, as a general result of the causes which determine the forms of living things. When these causes or principles, of whatever nature they are conceived to be, vary so as to modify one part of the organization of the being, they also modify another: and thus the groups which exhibit this variation of the fundamental principles of form, are the same, whether the manifestation of the change be sought in one part or in another of the organized structure. The groups thus formed are related by Affinity; and in proportion as we find the evidence of more functions and more organs to the propriety of our groups, we are more and more satisfied that they are Natural Classes. It appears, then, that our Idea of Affinity involves the conviction of the *Coincidence of natural arrangements formed on different functions;* and this, rather than the principle of the Subordination of some characters to others, is the true ground of the natural method of Classification.

5. For example, Cuvier, after speaking of the Subordination of Characters as the guide which he intends to follow in his arrangement of animals, interprets this principle in such a manner[3] as to make it agree nearly with the one just stated: 'In pursuance of what has been said on methods in general, we now require to

[3] *Règne Animal*, p. 55.

know what characters in animals are the most influential, and therefore those which must be made the grounds of the primary divisions.' 'These,' he says, 'it is clear must be those which are taken from the animal functions;—sensation and motion:'—But how does he confirm this? Not by showing that the animal functions are independent of, or predominant over, the vegetative, but by observing that they follow the same gradations. 'Observation,' he continues, 'confirms this view, by showing that the degrees of development and complication of the animal functions agree with those of the vegetative. The heart and the organs of the circulation are a sort of center for the vegetative functions, as the brain and the trunk of the nervous system are for the animal functions. Now we see these two systems descend in the scale, and disappear the one with the other. In the lowest animals, when there are no longer any distinct nerves, there are also no longer distinct fibres, and the organs of digestion are simply hollowed out in the homogeneous mass of the body. The muscular system disappears even before the nervous, in insects; but in general the distribution of the medullary masses corresponds to that of the muscular instruments; a spinal cord, on which knots or ganglions represent so many brains, corresponds to a body divided into numerous rings and supported on pairs of members placed at different points of the length, and so on.

'This *correspondence* of the general forms which result from the arrangement of the motive organs, from the distribution of the nervous masses, and from the energy of the circulatory system, must therefore form the ground of the first great sections by which we divide the animal kingdom.'

6. Decandolle takes the same view. There must be, he says, *an equilibrium* of the different functions[4]. And he exemplifies this by the case of the distinction of monocotyledonous and dicotyledonous plants, which being at first established by means of the organs of re-

[4] *Theor. Elem.* p. 79.

production, was afterwards found to coincide with the distinction of endogenous and exogenous, which depends on the process of nutrition. 'Thus,' he adds, '*the natural classes founded on one of the great functions of the vegetable are necessarily the same as those which are founded upon the other function;* and I find here a very useful *criterion* to ascertain whether a class is natural: namely, in order to announce that it is so, it must be arrived at by the two roads which vegetable organization presents. Thus I affirm,' he says, 'that the division of monocotyledons from dicotyledons, and the distinction of Gramineæ from Cyperaceæ, are real, because in these cases, I arrive at the same result by the reproductive and the nutritive organs; while the distinction of monopetalous and polypetalous, of Rhodoraceæ and Ericineæ, appears to me artificial, because I can arrive at it only by the reproductive organs.'

Thus the Correspondence of the indications of different functions is the criterion of Natural Classes; and this correspondence may be considered as one of the best and most characteristic marks of the fundamental Idea of Affinity. And the Maxim by which all Systems professing to be natural must be tested is this:—that the *arrangement obtained from one set of characters coincides with the arrangement obtained from another set.*

This Idea of Affinity, as a natural connexion among various species, of which connexion all particular resemblances are indications, has principally influenced the attempts at classifying the *animal* kingdom. The reason why the classification in this branch of Natural History has been more easy and certain than that of the vegetable world is, as Decandolle says[5], that besides the functions of nutrition and reproduction, which animals have in common with plants, they have also in addition the function of sensation; and thus have a new means of verification and concordance. But we may add, as a further reason, that the functions of

[5] *Theor. Elem.* p. 80.

animals are necessarily much more obvious and intelligible to us than those of vegetables, from their clear resemblance to the operations which take place in our own bodies, to which our attention has necessarily been strongly directed.

7. The question here offers itself, whether this Idea of Natural Affinity is applicable to inorganic as well as to organic bodies;—whether there be Natural Affinities among Minerals. And to this we are now enabled to reply by considering whether or not the principle just stated is applicable in such cases. And the conclusion to which our principle leads us is,—that there are such Natural Affinities among Minerals, since there are different sets of characters which may be taken, (and have by different writers been taken,) as the basis of classification. The hardness, specific gravity, colour, lustre, crystallization, and other *external* characters, as they are termed, form one body of properties according to which minerals may be classified; as has in fact been done by Mohs, Breithaupt, and others. The *chemical* constitution of the substances, on the other hand, may be made the principle of their arrangement, as was done by Haüy, and more recently, and on a different scheme, by Berzelius. Which of these is the true and natural classification? To this we answer, that *each* of these arrangements is true and natural, then, and then only, when it coincides with the other. An arrangement by external characters which gives us classes possessing a common chemical character;—a chemical order which brings together like and separates unlike minerals;—such classifications have the evidence of truth in their agreement with one another. Every classification of minerals which does not aim at and tend to such a result, is so far merely arbitrary; and cannot be subservient to the expression of general chemical and mineralogical truths, which is the proper purpose of such a classification.

8. In the History of Mineralogy I have related the advances which have been made among mineralogists and chemists in modern times towards a System

possessing this character of truth. I have there described the mixed systems of Werner and Haüy;—the attempt made by Mohs to form a pure Natural History system;—the first and second attempt of Berzelius to form a pure chemical system; and the failure of both these attempts. But the distinct separation of the two elements of which science requires the coincidence threw a very useful light upon the subject; and the succeeding mixed systems, such as that of Naumann, approached much nearer to the true conditions of the problem than any of the preceding ones had done. Thus, as I have stated, several of Naumann's groups have both a common chemical character and great external resemblances. Such are his *Anhydrous Unmetallic Haloids*—his *Anhydrous Metallic Haloids*—*Hydrous Metallic Haloids*—*Oxides* of metals—*Pyrites*—*Glances*—*Blendes.* The existence of such groups shows that we may hope ultimately to obtain a classification of minerals which shall be both chemically significant, and agreeable to the methods of Natural History: although when we consider how very imperfect as yet our knowledge of the chemical composition of minerals is, we can hardly flatter ourselves that we shall arrive at such a result very soon.

We have thus seen that in Mineralogy, as well as in the sciences which treat of organized bodies, we may apply the Idea of Natural Affinity; of which the fundamental maxim is, that *arrangements obtained from different sets of characters must coincide.*

Since the notion of Affinity is thus applicable to inorganic as well as to organic bodies, it is plain that it is not a mere modification of the Idea of Organization or Function, although it may in some of its aspects appear to approach near to these other Ideas. But these Ideas, or others which are the foundation of them, necessarily enter in a very prominent and fundamental manner into all the other parts of Natural History. To the consideration of these, therefore, we shall now proceed.

BOOK IX.

THE

PHILOSOPHY

OF

BIOLOGY.

La vie est donc un Tourbillon plus ou moins rapide, plus ou moins compliqué, dont la direction est constante, et qui entraine toujours des molecules de mêmes sorts, mais où les molecules individuelles entrent et d'où elles sortent continuellement, de manière que la *Forme* du corps vivant lui est plus essentielle que sa *Matière.*

Tant que ce mouvement subsiste, le corp où il s'exerce est *vivant; il vit.* Lorsque le mouvement s'arrête sans retour, le corps *meurt.*

Cuvier, *Règne Animal,* s. 12.

I remember, upon asking our famous Harvey, what induced him to think of a circulation of the blood, he said, that observing the valves in the veins of many parts of the body, so placed as to give a free passage to the blood towards the heart, but to oppose the passage of the venal blood the contrary way, he imagined that so provident a cause as nature had not thus placed so many valves without design; and as no design seemed more probable than that the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent through the arteries and return through the veins when valves did not oppose its course that way.

Boyle, *On the Final Causes of Natural Things.* On the Proposition: *'Tis often allowable for a naturalist, from the manifest and apposite uses of the parts of animal bodies, to collect some of the particular ends for which the Creator designed them: and in some cases we may, from the known nature and structure of the parts, draw particular conjectures about the particular offices of them.*

BOOK IX.

THE PHILOSOPHY OF BIOLOGY.

CHAPTER I.

ANALOGY OF BIOLOGY WITH OTHER SCIENCES.

1. IN the History of the Sciences, after treating of the Sciences of Classification, we proceeded to what are there termed the Organical Sciences, including in this term Physiology and Comparative Anatomy. A peculiar feature in this group of sciences is that they involve the notion of *living* things. The notion of *Life*, however vague and obscure it may be in men's minds, is apprehended as a peculiar Idea, not resolvable into any other Ideas, such, for instance, as Matter and Motion. The separation between living creatures and inert matter, between organized and unorganized beings, is conceived as a positive and insurmountable barrier. The two classes of objects are considered as of a distinct kind, produced and preserved by different forces. Whether the Idea of Life is really thus original and fundamental, and whether, if so, it be one Idea only, or involve several, it must be the province of true philosophy to determine. What we shall here offer may be considered as an attempt to contribute something to the determination of these questions; but we shall perhaps be able to make it appear that science is at present only in the course of its progress towards a complete solution of such problems.

Since the main feature of those sciences of which we have now to examine the philosophy is, that they

involve the Idea of Life, it would be desirable to have them designated by a name expressive of that circumstance. The word *Physiology*, by which they have most commonly been described, means *the Science of Nature;* and though it would be easy to explain, by reference to history, the train of thought by which the word was latterly restricted to *Living Nature*, it is plain that the name is, etymologically speaking, loose and improper. The term *Biology*, which means exactly what we wish to express, *the Science of Life*, has often been used, and has of late become not uncommon among good writers. I shall therefore venture to employ it, in most cases, rather than the word *Physiology*.

2. As I have already intimated, one main inquiry belonging to the Philosophy of Biology, is concerning the Fundamental Idea or Ideas which the science involves. If we look back at the course and the results of our disquisitions respecting other sciences in this work, and assume, as we may philosophically do, that there will be some general analogy between those sciences and this, in their development and progress, we shall be enabled to anticipate in some measure the nature of the view which we shall now have to take. We have seen that in other subjects the Fundamental Ideas on which science depended, and the Conceptions derived from these, were at first vague, obscure, and confused;—that by gradual steps, by a constant union of thought and observation, these conceptions become more and more clear, more and more definite;—and that when they approached complete distinctness and precision, there were made great positive discoveries into which these conceptions entered; and thus the new precision of thought was fixed and perpetuated in some conspicuous and lasting truths. Thus we have seen how the first confused mechanical conceptions (Force, and the like,) were, from time to time, growing clearer, down to the epoch of Newton;—how true conceptions of Genera and of wider classes, gradually unfolded themselves among the botanists of the sixteenth and seventeenth centuries;—how the idea of Substance became steady enough to govern the

theories of chemists only at the epoch of Lavoisier;—how the Idea of Polarity, although often used by physicists and chemists, is even now somewhat vague and indistinct in the minds of the greater part of speculators. In like manner we may expect to find that the Idea of Life, if indeed *that* be the governing Idea of the Science which treats of Living Things, will be found to have been gradually approaching towards a distinct and definite form among the physiologists of all ages up to the present day. And if this be the case, it may not be considered superfluous, with reference to so interesting a subject, if we employ some space in tracing historically the steps of this progress;—the changes by which the originally loose notion of Life, or of Vital Powers, became more nearly an Idea suited to the purposes of science.

3. But we may safely carry this analogy between Biology and other sciences somewhat further. We have seen, in other sciences, that while men in their speculations were thus tending towards a certain peculiar Idea, but before they as yet saw clearly that it was peculiar and independent, they naturally and inevitably clothed their speculations in conceptions borrowed from some other extraneous idea. And the unsatisfactoriness of all such attempts, and the necessary consequence of this, a constant alteration and succession of such inappropriate hypotheses, were indications and aids of the progress which was going on towards a more genuine form of the science. For instance, we have seen that in chemistry, so long as men refused to recognize a peculiar and distinct kind of power in the *Affinity* which binds together the elements of bodies, they framed to themselves a series of hypotheses, each constructed according to the prevalent ideas of the time, by which they tried to represent the relation of the compound to the ingredients:—first, supposing that the elements bestowed upon the whole qualities *resembling* their own:—then giving up this supposition, and imagining that the properties of the body depended upon the *shape* of the component particles;—then, as their view expanded, assuming that it was

not the shape, but the mechanical *forces* of the particles which gave the body its attributes;—and finally acquiescing in, or rather reluctantly admitting, the idea of *Affinity*, conceived as a peculiar power, different not only from material contact, but from any mechanical or dynamical attraction.

Now we cannot but think it very natural, if we find that the history of Biology offers a series of occurrences of the same nature. The notions of Life in general, or of any Vital Functions or Vital Forces in particular, are obviously very loose and vague as they exist in the minds of most men. The discrepancies and controversies respecting the definitions of all such terms, which are found in all works on physiology, afford us abundant evidence that these notions are not, at least not generally, apprehended with complete clearness and steadiness. We shall therefore find approaches and advances, intermediate steps, gradually leading up to the greatest degree of distinctness which has yet been attained. And in those stages of imperfect apprehension in which the notions of Life and of Vital Powers are still too loose and unformed to be applied independently, we may expect to find them supported and embodied by means of hypotheses borrowed from other subjects, and thus, made so distinct and substantial as to supply at least a temporary possibility of scientific reasoning upon the laws of life.

4. For example, if we suppose that men begin to speculate upon the properties of living things, not acknowledging a peculiar Vital Power, but making use successively of the knowledge supplied by the study of other subjects, we may easily imagine a series of hypotheses along which they would pass.

They would probably, first, in this as in other sciences, have their thoughts occupied by vague and *mystical* notions in which material and spiritual agency, natural and supernatural events, were mixed together without discrimination, and without any clear notion at all. But as they acquired a more genuine perception of the nature of knowlege, they would naturally try to explain vital motions and processes by means of

such forces as they had learnt the existence of from other sciences. They might first have a *mechanical* hypothesis, in which the mechanical *Forces* of the solids and fluids which compose organized bodies should be referred to, as the most important influences in the process of life. They might then attend to the actions which the fluids exercise in virtue of their *Affinity*, and might thus form a *chemical* theory. When they had proved the insufficience of these hypotheses, borrowed from the powers which matter exhibits in other cases, they might think themselves authorized to assume some peculiar power or agency, still material, and thus they would have the hypothesis of a *Vital Fluid*. And if they were driven to reject this, they might think that there was no resource but to assume an immaterial principle of life, and thus they would arrive at the doctrine of an *Animal Soul*.

Now, through the cycle of hypotheses which we have thus supposed, physiology has actually passed. The conclusions to which the most philosophical minds have been led by a survey of this progress is, that by the failure of all these theories, men have exhausted this path of inquiry, and shown that scientific truth is to be sought in some other manner. But before I proceed further to illustrate this result, it will be proper, as I have already stated, to exhibit historically the various hypotheses which I have described. In doing this I shall principally follow the *History of Medicine* of Sprengel. It is only by taking for my guide a physiologist of acknowledged science and judgment, that I can hope, on such a subject, to avoid errours of detail. I proceed now to give in succession an account of the Mystical, the Iatrochemical, the Iatromathematical, and the Vital-Fluid Schools; and finally of the Psychical School, who hold the Vital Powers to be derived from the Soul (*Psychè*).

CHAPTER II.

SUCCESSIVE BIOLOGICAL HYPOTHESES.

SECT. I.—*The Mystical School.*

IN order to abbreviate as much as can conveniently be done the historical view which I have now to take, I shall altogether pass over the physiological speculations of the ancients, and begin my survey with the general revival of science in modern times.

We need not dwell long on the fantastical and unsubstantial doctrines concerning physiology which prevailed in the sixteenth century, and which flowed in a great measure from the fertile but ill-regulated imaginations of the cultivators of Alchemy and Magic. One of the prominent doctors of this school is the celebrated Paracelsus, whose doctrines contained a combination of biblical interpretations, visionary religious notions, fanciful analogies, and bold experiments in practical medicine. The opinion of a close but mystical resemblance of parts between the universe and the human body,—the *Macrocosm* and the *Microcosm*,—as these two things, thus compared, were termed, had probably come down from the Neoplatonists; it was adopted by the Paracelsists[1], and connected with various astrological dreams and cabbalistic riddles. A succession of later Paracelsists[2], Rosicrucians, and other fanatics of the same kind, continued into the seventeenth century. Upon their notions was founded the pretension of curing wounds by a sympathetic powder, which Sir Kenelm Digby, among others, asserted; while animal magnetism, and the transfer of diseases from one person to another[3], were maintained by others of this

[1] Spr. iii. 456. [2] *Ib.* iv. 270. [3] *Ib.* iv. 276.

school. They held, too, the doctrines of *astral bodies* corresponding to each terrestrial body; and of the *signatures* of plants, that is, certain features in their external form by which their virtues might be known. How little advantage or progress real physiology could derive from speculations of this kind may be seen from this, that their tendency was to obliterate the distinction between living and lifeless things: according to Paracelsus, all things are alive, eat, drink, and excrete; even minerals and fluids[4]. According to him and his school, besides material and immaterial beings, there are *elementary Spirits* which hold an intermediate place, *Sylvans, Nymphs, Gnomes, Salamanders*, &c. by whose agency various processes of enchantment may be achieved, and things apparently supernatural explained. Thus this spiritualist scheme dealt with a world of its own by means of fanciful inventions and mystical visions, instead of making any step in the study of nature.

Perhaps, however, one of the most fantastical of the inventions of Paracelsus may be considered as indicating a perception of a peculiar character in the vital powers. According to him, the business of digestion is performed by a certain demon whom he calls *Archæus*, who has his abode in the stomach, and who, by means of his alchemical processes, separates the nutritive from the harmful part of our food, and makes it capable of assimilation[5]. This fanciful notion was afterwards adopted and expanded by Van Helmont[6]. According to him the stomach and spleen are both under the direction of this Master-spirit, and these two organs form a sort of *Duumvirate* in the body.

But though we may see in such writers occasional gleams of physiological thought, the absence of definite physical relations in the speculations thus promulgated was necessarily intolerable to men of sound understanding and scientific tendencies. Such men naturally took hold of that part of the phenomena of life which could be most distinctly conceived, and

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[4] Spr. iii. 458. Parac. *De Vita Rerum Naturalium*, p. 889.

[5] *Ib.* iii. 468.

[6] *Ib.* iv. 302.

which could be apparently explained by means of the sciences then cultivated; and this was the part which appeared to be reducible to chemical conceptions and doctrines. It will readily be supposed that the processes of chemistry have a considerable bearing upon physiological processes, and might, till their range was limited by a sound investigation, be supposed to have still more than they really had; and thus a Physiology was formed which depended mainly upon Chemistry, and the school which held this doctrine has been called the *Iatrochemical* School.

SECT. II.—*The Iatrochemical School.*

That all physical properties, and therefore chemical relations, have a material influence on physiological results, was already recognized, though dimly, in the Galenic doctrine of the 'four elementary qualities.' But at the time of Paracelsus, chemical action was more distinctly than before separated from other kinds of physical action; and therefore a physiological doctrine, founded upon chemistry, and freed from the extravagance and mysticism of the Paracelsists, was a very promising path of speculation. Andrew Libavius[7] of Halle, in Saxony, Physician and Teacher in the Gymnasium at Koberg, is pointed out by Sprengel as the person who began to cultivate chemistry, as distinct from the theosophic fantasies of his predecessors; and Angelus Sala of Vienna[8], as his successor. The latter has the laudable distinction of having rejected the prevalent conceits about a potable gold, a universal medicine, and the like[9]. In Germany already at the beginning of the seventeenth century a peculiar chair of *Chymiatria* was already created at Marpurg: and many in various places pursued the same studies, till, in the middle of the seventeenth century, we come to Lemery[10], the principal reformer of pharmaceutical chemistry. But we are not here so much concerned

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[7] Spr. iii. 550.
[8] *Ib.* iv. 281.
[9] *Ib.* iv. 283.
[10] *Ib.* iv. 291.

with the practical as with the theoretical parts of Iatrochemistry; and hence we pass on to Sylvius[11] and his system.

The opinion that chemistry had an important bearing upon physiology did not, however, begin with Sylvius. Paracelsus, among his extravagant absurdities, did some service to medicine by drawing attention to this important truth. He used[12] chemical principles for the explanation of particular diseases: most or all diseases according to him, arise from the effervescence of salts, from the combustion of sulphur, or from the coagulation of mercury. His medicines were chemical preparations; and it was[13] an undeniable advantage of the Paracelsian doctrine that chemistry thus became indispensable to the physician. We still retain a remnant of the chemical nomenclature of Paracelsus in the term *tartar*, denoting the stony concretion which forms on the teeth[14]. According to him there is a certain substance, the basis of all diseases which arise from a thickening of the juices and a collection of earthy matter; and this substance he calls *Tartarus*, because it 'burns like the fire of hell.' Helmont, the successor of Paracelsus in many absurdities, also followed him in the attempt to give a chemical account, however loose and wild, of the functions of the human body; and is by Sprengel considered, with all his extravagancies, as a meritorious and important discoverer. The notion of the fermentation of fluids[15], and of the aërial product thence resulting, to which he gave the name of *Gas*, forms an important part of his doctrines; and of the six digestions which he assumes, the *first* prepares an acid, which is neutralized by the gall when it reaches the duodenum, and this constitutes the *second* digestion.

I have already, in the History of Chemistry[16], stated, that the doctrine of the opposition of acid and alkali, the great step which theoretical chemistry owes to Sylvius, was first brought into view as a physiological

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11 Spr. iv. 336. 12 *Ib.* iii. 472. 13 *Ib.* iii. 482. 14 *Ib.* iii. 475.
15 Vol. v. 315. 16 *Hist. Ind. Sc.* b. xiii. c. 2.

tenet, although we had then to trace its consequences in another science. The explanation of all the functions of the animal system, both healthy and morbid, by means of this and other chemical doctrines, and the prescription of methods of cure founded upon such explanations, form the scheme of the *iatrochemical* school; a school which almost engrossed the favour of European physicians during the greater part of the seventeenth century.

Sylvius taught medicine at Leyden, from the year 1658, with so much success, that Boerhaave alone surpassed him[17]. His notions, although he piqued himself on their originality, were manifestly suggested in no small degree (as all such supposed novelties are) by the speculations of his predecessors, and the spirit of the times. Like Helmont[18], he considers digestion as consisting in a fermentation; but he states it more definitely as the effervescence of an acid, supplied by the saliva and the pancreatic juice, with the alkali of the gall. By various other hypothetical processes, all of a chemical nature, the blood becomes a collection of various juices, which are the subjects of the speculations of the iatrochemists, to the entire neglect of the solid parts of the body. Diseases were accounted for by a supposed prevalence of one or the other of the acrid principles, the acid or the alkaline: and Sylvius[19] was bold enough to found upon these hypotheses practical methods of cure, which were in the highest degree mischievous.

The Sylvian doctrine was often combined with some of the notions of the Cartesian system of philosophy; but this mixture I shall not notice, since my present object is to trace the history of a mere chemical physiology as one of the unsuccessful attempts at a philosophy of life. With various modifications, this doctrine was diffused over Europe. It gave rise to several controversies, which turned upon the questions of the novelty of the doctrine, and the use of chemical remedies to which it pointed, as well as upon its theo-

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[17] Spr. iv. 336. [18] *Ib.* 338. [19] *Ib.* iv. 345.

retical truth. We need not dwell long upon these controversies, although they were carried on with no small vehemence in their time. Thus the school of Paris opposed all innovation, remained true to the Galenic dogmatism, and declared itself earnestly against all combination of chemistry with medicine; and even against the chemical preparation of medicaments. Guy Patin, a celebrated and learned professor of that day, declares[20] that the chemists are no better than forgers, and ought to be punished as such. The use of antimonial medicines was a main point of dispute between the iatrochemists and their opponents; Patin maintained that more men had been destroyed by antimony than by the thirty years' war of Germany; and endeavoured to substantiate this assertion by collecting all such cases in his *Martyrologium Antimonii.* It must have been a severe blow to Patin when[21] in 1666, the Doctors of the Faculty of Paris, assembled by command of the parliament, declared, by a majority of ninety-two voices, that the use of antimonial medicines was allowable and laudable, and when all attempts to set aside this decision failed.

Florentius Schuyl of Leyden sought to recommend the iatrochemical doctrines, by maintaining that they were to be found in the Hippocratic writings; nor was it difficult to give a chemical interpretation of the humoral pathology of the ancients. The Italian[22] physicians also, for the most part, took this line, and attempted to show the agreement of the principles of the ancient school of medicine with the new chemical notions. This, indeed, is the usual manner in which the diffusion of new theoretical ideas becomes universal.

The progress of the chemical school of medicine in England[23] requires our more especial notice. Willis was the most celebrated champion of this sect. He assumed, but with modifications of his own, the three Paracelsian principles, Salt, Sulphur, and Mercury; considered digestion as the effect of an acid, and ex-

[20] Spr. 349. [21] *Ib.* iv. 350. [22] *Ib.* 368. [23] *Ib.* 353.

plained other parts of the animal economy by distillation, fermentation, and the like. All diseases arise from the want of the requisite *ferment;* and the physician, he says[24], may be compared to a vintner, since both the one and the other have to take care that the necessary fermentations go on, that no foreign matter mixes itself with the wine of life, to interrupt or derange those operations. In the middle of the seventeenth century, medicine had reached a point in which the life of the animal body was considered as merely a chemical process; the wish to explain everything on known principles left no recognized difference between organized and unorganized bodies, and diseases were treated according to this delusive notion. The condition of chemistry itself during this period, though not one of brilliant progress, was sufficiently stable and flourishing to give a plausibility to any speculation which was founded on chemical principles; and the real influence of these principles in the animal frame could not be denied.

The iatrochemists were at first resisted, as we have seen, by the adherents of the ancient schools; they were attacked on various grounds, and finally deposed from their ascendancy by another sect, which we have to speak of, as the iatromathematical, or mechanical school. This sect was no less unsatisfactory and erroneous in its positive doctrines than the chemists had been; for the animal frame is no more a mere machine than a mere laboratory: but it promoted the cause of truth, by detecting and exposing the insufficient explanations and unproved assertions of the reigning theory.

Boyle was one of the persons who first raised doubts against the current chemical doctrines of his time, as we have elsewhere noted; but his objections had no peculiar physiological import. Herman Conring[25], the most learned physician of his time, a contemporary with Sylvius, took a view more pertinent to our present object; for he not only rejected the alchemical

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[24] Spr. 354.

[25] *Ib.* iv. 361.

and hermetical medicines, but taught expressly that chemistry, in its then existing condition, was better fitted to be of use in the practice of pharmacy, than in the theories of physiology and pathology. He made the important assertion, also, that chemical principles do not pre-exist *as such* in the animal body; and that there are higher powers which operate in the organic world, and which do not depend on the form and mixture of matter.

Attempts were made to prove the acid and alkaline nature of the fluids of the human body by means of experiments, as by John Viridet of Geneva[26], and by Raimond Vieussens[27], the latter of whom maintained that he had extracted an acid from the blood, and detected a ferment in the stomach. In opposition to him, Hecquet, a disciple of the iatromathematical school, endeavoured to prove that digestion was performed, not by means of fermentation, but by trituration. Hecquet's own opinions cannot be defended; but his objections to the chemical doctrines, and his assertion of the difference of chemical and organical processes, are evidences of just thought[28].

The most important opponents of the iatrochemical school were Pitcairn in England, Bohn and Hoffman in Germany, and Boerhaave in Holland. These eminent physicians, about the end of the seventeenth century, argued on the same grounds of observation, that digestion is not fermentation, and that the Sylvian accounts of the origin of diseases by means of acid and alkali are false. The arguments and authority of these and other persons finally gained an ascendancy in the medical world, and soon after this period we may consider the reign of the chemical school of physiology as past. In fact, the attempts to prove its assertions experimentally were of the feeblest kind, and it had no solid basis on which it could rest, so as to resist the shock of the next hypothesis which the progress of the physical sciences might impel against it. We may, therefore, now consider the opinion of the mere

[26] Spr. iv. 329. [27] *Ib.* 350, (1715). [28] *Ib.* 401.

chemical nature of the vital processes as disproved, and we proceed next to notice the history of another unsuccessful essay to reduce vital actions to known actions of another kind.

Sect. III.—*The Iatromathematical School.*

In the first Section of this chapter, we enumerated the biological hypotheses which at first present themselves, as the mystical, the mechanical, the chemical. We might have expected that they should occur to men's minds in the order thus stated: and in fact they did so; for the physiology of the ancient materialists, as Democritus and Lucretius, is mechanical so far as it is at all distinct in its views, and thus the mechanical preceded the chemical doctrine. But in modern times, the fluid or chemical physiology was developed before the solid or mechanical: of which the reason appears to have been this;—that Mechanics and Chemistry began to assume a scientific character about the same time; and that of the two, Chemistry not only appeared at first sight more applicable to the functions of the body, because all the more rapid changes appear to be connected with modifications of the fluids of the animal system, but also, by its wider range of facts and more indefinite principles, afforded a better temporary refuge for the mind when perplexed by the difficulties and mysteries which spring out of the speculations concerning life. But if Chemistry was thus at first a more inviting field for the physiologist, Mechanics soon became more attractive in virtue of the splendid results obtained by the schools of Galileo and Newton. And when the insufficiency of chemical physiology was discovered by trial, as we have seen it was, the hope naturally arose, that the mechanical principles which had explained so many of the phenomena of the external universe might also be found applicable to the smaller world of material life;—that the *microcosm* as well as the *macrocosm* might have its mechanical principles. From this hope sprung the

Iatromathematical School, or school of Mechanical Physiologists.

We may, however, divide this school into two parts, the Italian, and the Cartesio-Newtonian sect. The former employed themselves in calculating and analysing a number of the properties of the animal frame which are undoubtedly mechanical; the latter, somewhat intoxicated by the supposed triumphs of the corpuscular philosophy, endeavoured to extend these to physiology, and for this purpose introduced into the subject many arbitrary and baseless hypotheses. I will very briefly mention some of the writers of both these sects.

The main points to which the Italian or genuine Mechanical Physiologists attended, were the application of mechanical calculations to the force of the muscles, and of hydraulical reasonings to the motion of the fluids of the animal system. The success with which Galileo and his disciples had pursued these branches of mechanical philosophy, and the ascendancy which they had obtained, first in Italy, and then in other lands, made such speculations highly interesting. Borelli may be considered as the first great name in his line, and his book, *De Motu Animalium*, (*Opus Posthumum*, Romæ, 1680,) is even now a very instructive treatise on the forces and action of the bones and muscles. This, certainly one of the most valuable portions of mechanical physiology, has not even yet been so fully developed as it deserves, although John Bernoulli[29] and his son Daniel[30] applied to it the resources of analysis, and Pemberton[31] in England, pursued the same subject. Other of these mechanico-physiological problems consisted in referring the pressure of the blood and of the breath to hydrostatical principles. In this manner Borelli was led to assert that the muscles of the heart exert a force of 180,000 pounds[32]. But a little later, Keill reduced this force

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[29] *De Motu Musculorum.*

[30] *Act. Acad. Petrop.*

[31] *Course of Physiology*, 1773.

[32] Spr. iv. 110.

to a few ounces[33]. Keill and others attempted to determine, on similar principles, the velocity of the blood; we need not notice the controversies which thus arose, since there is not involved in them any peculiar physiological principle.

The peculiar character of the iatromathematical school, as an attempt at physiological theory, is more manifest in its other section, which we have called the Cartesio-Newtonian. The Cartesian system pretended to account for the appearances and changes of bodies by means of the size, figure, and motion of their minute particles. And though this system in its progress towards the intellectual empire of Europe was suddenly overturned by the rise of the Newtonian philosophy, these corpuscular doctrines rather gained than lost by the revolution; for the Newtonian philosophy enlarged the powers of the corpuscular hypothesis, by adding the effects of the attractive and repulsive forces of particles to those of their form and motion. By this means, although Newton's discoveries did not in fact augment the probability of the corpuscular hypothesis, they so far increased its plausibility, that this hypothesis found favour both with Newton himself and his contemporaries, no less than it had done with the Cartesians.

The attempt to apply this corpuscular hypothesis to physiology was made by Des Cartes himself. The general character of such speculations may easily be guessed[34]. The secretions are effected by the organs operating after the manner of sieves. Round particles pass through cylindrical tubes, pyramidal ones through triangular pores, cubical particles through square apertures, and thus different kinds of matter are separated. Similar speculations were pursued by other mathematicians: the various diameter of the vessels[35], their curvatures, folds, and angles, were made subjects of calculation. Bellini, Donzellini, Gulielmini, in Italy; Perrault, Dodart, in France; Cole, Keill, Jurin, in England, were the principal cultivators of such studies.

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[33] Spr. iv. 443. [34] *Ib.* 329. [35] *Ib.* 432.

In the earlier part of the eighteenth century, physiological theorists considered it as almost self-evident that their science required them to reason concerning the size and shape of the particles of the fluids, the diameter and form of the invisible vessels. Such was, for instance, the opinion of Cheyne[36], who held that acute fevers arise from the obstruction of the glands, which occasions a more vehement motion of the blood. Mead, the physician of the King, and the friend of Newton, in like manner explained the effects of poisons by hypotheses concerning the form of their particles[37], as we have already seen in speaking of chemistry.

It is not necessary for us to dwell longer on this subject, or to point out the total insufficiency of the mere mechanical physiology. The iatrochemists had neglected the effect of the solids of the living frame; the iatromathematicians attended only to these[38]. And even these were considered only as canals, as cords, as levers, as lifeless machines. These reasoners never looked for any powers of a higher order than the cohesion, the resistance, the gravity, the attraction, which operate in inert matter. If the chemical school assimilated the physician to a vintner or brewer, the mechanical physiologists made him an hydraulic engineer; and, in fact, several of the iatromathematicians were at the same time teachers of engineering and of medicine.

Several of the reasoners of this school combined chemical with their mechanical principles; but it would throw no additional light upon the subject to give any account of these, and I shall therefore go on to speak of the next form of the attempt to explain the processes of life.

SECT. IV.—*The Vital-Fluid School.*

I speak here, not of that opinion which assumes some kind of fluid or ether as the means of communi-

[36] Spr. iv. 223. [37] *Mechanical Account of Poisons*, 1702.
[38] Spr. iv. 419.

cation along the *nerves* in particular, but of the hypothesis that *all* the peculiar functions of *life* depend upon some subtile ethereal substance diffused through the frame;—not of a *Nervous* Fluid, but of a *Vital* Fluid. Again, I distinguish this opinion from the doctrine of an *immaterial* vital power or principle, an Animal Soul, which will be the subject of the next Section: nor is this distinction insignificant; for a material element, however subtile, however much spiritualized, must still act everywhere according to the same laws; whereas we do not conceive an immaterial spirit or soul to be subject to this necessity.

The iatromathematical school could explain to their own satisfaction how motions, once begun, were transferred and modified; but in many organs of the living frame there seemed to be a power of beginning motion, which is beyond all mere mechanical action. This led to the assumption of a Principle of a higher kind, though still material. Such a Principle was asserted by Frederick Hoffmann, who was born at Halle, in 1660[39], and became Professor of Medicine at the newly established University there in 1694. According to him[40], the reason of the greater activity of organized bodies lies in the influence of a material substance of extreme subtilty, volatility, and energy. This is, he holds, no other than the Ether, which, diffused through all nature, produces in plants the bud, the secretion and motion of the juices, and is separated from the blood and lodged in the brain of animals[41]. From this, acting through the nerves, must be derived all the actions of the organs in the animal frame; for when the influence of the nerve upon the muscle ceases, muscular motion ceases also.

The mode of operation of this vital fluid was, however, by no means steadily apprehended by Hoffmann and his followers. Its operations are so far mechanical[42] that all effects are reduced to motion, yet they

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[39] Spr. v. 254. [40] *Ib.* v. 257.

[41] *De Differentiâ Organismi et Mechanismi*, pp. 48, 67.

[42] Spr. v. 262, 3.

cannot be explained according to known mechanical laws. At one time the effects are said to take place according to laws of a Higher Mechanics which are still to be discovered[43]. At another time, in complete contradiction of the general spirit of the system, metaphysical conceptions are introduced: each particle of the vital fluid is said to have a determined *idea* of the whole mechanism and organism[44], and according to this, it forms the body and preserves it by its motion. By means of this fluid the soul operates upon the body, and the instincts and the passions have their source in this material sensitive soul. This attribution of ideas to the particles of the fluid is less unaccountable when we recollect that something of the same kind is admitted into Leibnitz's system, whose Monads have also ideas.

Notwithstanding its inconsistencies, Hoffmann's system was received with very general favour both in Germany and in the rest of Europe; the more so, inasmuch as it fell in very well with the philosophy both of Leibnitz and of Newton. The Newtonians were generally inclined to identify the Vital Fluid with the Ether, of which their master was so strongly disposed to assume the existence: and indeed he himself suggested this identification.

When the discoveries made respecting Electricity in the course of the eighteenth century had familiarized men with the notion of a pervading subtile agent, invisible, intangible, yet producing very powerful effects in every part of nature, physiologists also caught at the suggestion of such an agent, and tried, by borrowing or imitating it, to aid the imperfection of their notions of the vital powers. The Vital Principle[45] was imagined to be a substance of the same kind, by some to be the same substance, with the Electric Fluid. By its agency all these processes in organized bodies were accounted for which cannot be

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[43] Hoffmann, *Opp.* Vol. v. p. 123.

[44] *De Diff. Organ. et Mechan.* p. 81.

[45] Prichard, *On the Doctrine of a Vital Principle*, p. 12.

explained by mechanical or chemical laws, as the secretion of various matters (tears, milk, bile, &c.) from an homogeneous fluid, the blood; the production of animal heat, digestion, and the like. According to John Hunter, this attenuated substance pervaded the blood itself, as well as the solid organic frame; and the changes which take place in the blood which has flowed out of the veins into a basin are explained by saying that it is, for a time, till this vital fluid evaporates, truly alive.

The notion of a Vital Fluid appears also to be favourably looked upon by Cuvier; although with him this doctrine is mainly put forwards in the form of a Nervous Fluid. Yet in the following passage he extends the operation of such an agent to all the vital functions[46]: 'We have only to suppose that all the medullary and nervous parts produce the Nervous Agent, and that they alone conduct it; that is, that it can only be transmitted by them, and that it is changed or consumed by their actions. Then everything appears simple. A detached portion of muscle preserves for some time its irritability, on account of the portion of nerve which always adheres to it. The sensibility and the irritability reciprocally exhaust each other by their exercise, because they change or consume the same agent. All the interior motions of digestion, secretion, excretion, participate in this exhaustion, or may produce it. All local excitation of the nerves brings thither more blood by augmenting the irritability of the arteries, and the afflux of blood augments the real sensibility by augmenting the production of the nervous agent. Hence the pleasures of titillations, the pains of inflammation. The particular sensations increase in the same manner and by the same causes; and the imagination exercises, (still by means of the nerves,) upon the internal fibres of the arteries or other parts, and through them on the sensations, an action analogous to that of the will upon the voluntary motions. As each exterior sense is exclusively disposed

[46] *Hist. Sc. Nat. depuis* 1789, i. 214.

to admit the substances which it is to perceive, so each interior organ, secretory or other, is also more excitable by some one agent than by another: and hence arises what has been called the *proper sensibility* or *proper life of the organs;* and the influence of specifics which, introduced into the general circulation, affect only certain parts. In fine, if the nervous agent cannot become sensible to us, the reason is that all sensation requires that this agent should be altered in some way or other; and it cannot alter itself.

'Such is the summary idea which we may at present form of the mutual and general working of the vital powers in animals.'

Against the doctrine of a Vital Fluid as one uniform material agent pervading the organic frame, an argument has been stated which points out extremely well the philosophical objection to such an hypothesis[47]. If the Vital Principle be the *same* in all parts of the body, how does it happen, it is asked, that the secretions are so *different?* How do the particles in the blood, separated from their old compounds and united into new ones, under the same influence, give origin to all the different fluids which are produced by the glands? The liver secretes bile, the lacrymal gland, tears, and so on. Is the Vital Principle different in all these organs? To assert this, is to multiply nominal principles without limit, and without any advance in the explanation of facts. Is the Vital Principle the same, but its operation modified by the structure of the organ? We have then two unknown causes, the Vital Principle and the Organic Structure, to account for the effect. By such a multiplication of hypotheses nothing is gained. We may as well say at once, that the structure of the organ, acting by laws yet unknown, is the cause of the peculiar secretion. It is as easy to imagine this structure acting to produce the whole effect, as it is to imagine it modifying the activity of another agent. Thus the hypothesis of the Vital Fluid in this form explains nothing, and does not in any

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[47] Prichard, *On a Vital Principle*, p. 98.

way help onwards the progress of real biological knowledge.

The hypothesis of an *immaterial* vital principle must now be considered.

Sect. V.—*The Psychical School.*

The doctrine of an Animal Soul as the principle which makes the operations of organic different from those of inorganic matter, is quite distinct from, and we may say independent of, the doctrine of the soul as the intelligent, moral, responsible part of man's nature. It is the former doctrine alone of which we have here to speak, and those who thus hold the existence of an immaterial agent as the cause of the phenomena of life, I term the *Psychical School.*

Such a view of the constitution of living things is very ancient. For instance, Aristotle's Treatise '*On the Soul*,' goes entirely upon the supposition that the Soul is the cause of motion, and he arrives at the conclusion that there are different *parts* in the Soul; the *nutritive* or *vegetative*, the *sensitive*, and the *rational*[48].

But this doctrine is more instructive to us, when it appears as the antagonist of other opinions concerning the nature of life. In this form it comes before us as promulgated by Stahl, whom we have already noticed as one of the great discoverers in chemistry. Born in the same year as Hoffmann, and appointed at his suggestion professor at the same time in the same new university of Halle, he soon published a rival physiological theory. In a letter to Lucas Schröck, the president of the Academy of Naturalists, he describes the manner in which he was led to form a system for himself[49]. Educated in the tenets of Sylvius and Willis, according to which all diseases are derived from the acidity of the fluids, Stahl, when a young student, often wondered how these fluids, so liable to be polluted and corrupted, are so wonderfully preserved through innumerable external influences, and seem to

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[48] Arist. Περὶ Ψυχῆς, ii. 2.

[49] Spr. v. 303.

be far less affected by these than by age, constitution, passion. No material cause could, he thought, produce such effects. No attention to mechanism or chemistry alone could teach us the true nature and laws of organization.

So far as Stahl recognized the influence, in living bodies, of something beyond the range of mechanics and chemistry, there can be no doubt of the sound philosophy of his views; but when he proceeds to found a positive system of physiology, his tenets become more precarious. The basis of his theory is this[50]: the body has, as body, no power to move itself, and must always be put in motion by immaterial substances. All motion is a spiritual act[51]. The source of all activity in the organic body, from which its preservation, the permanency of its composition, and all its other functions proceed, is an immaterial being, which Stahl calls the *Soul;* because, as he says, when the effects are so similar, he will not multiply powers without necessity. Of this principle, he says, as the Hippocratians said of Nature, that 'it does without teaching what it ought to do[52],' and does it 'without consideration[53].' These ancient tenets Stahl interprets in such a manner that even the involuntary motions proceed from the soul, though without reflection or clear consciousness. It is indeed evident, that there are many customary motions and sensations which are perfectly rational, yet not the objects of distinct consciousness: and thus instinctive motions, and those of which we are quite unconscious, may still be connected with reason. The questions which in this view offer themselves, as, how the soul passes from the mother to the child, he dismisses as unprofitable[54]. He considers nutrition and secretion as the work of the soul. The corpuscular theory and the doctrine of animal spirits

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[50] Spr. v. 308.

[51] *Ib.* v. 314.

[52] Stahl, *περὶ φύσεως ἀπαίδευτου.*

[53] *οὐκ ἐκ διανοίης.*

[54] This was of course an obvious problem. Harvey, *On Generation* Exercise 27, p. 148, teaches, 'That the egg is not the production of the womb, but of the soul.'

are, he rightly observes, mere hypotheses, which are arbitrary in their character, and only shift the difficulty. For, if the animal spirits are not matter, how can they explain the action of an immaterial substance on the body; and if they are matter, how are they themselves acted on?

This doctrine of the action of the soul on the body, was accepted by many persons, especially by the iatro-mathematicians, who could not but feel the insufficiency of their system without some such supplement: such were Cheyne and Mead. In Germany, Stahl's disciples in physiology were for the most part inconsiderable persons[55]. Several Englishmen who speculated concerning the metaphysics as well as the physiology of Sensation and Motion, inclined to this psychical view, as Porterfield and Whytt. Among the French, Boissier de Sauvages was the most zealous defender of the Stahlian system. Actions, he says[56], which belong to the preservation of life are determined by a moral not a mechanical necessity. They proceed from the soul, but cannot be controlled by it, as the starting from fear, or the trembling at danger. Unzer, a physician at Altona[57], was also a philosophical Stahlian[58].

We need not dwell on the opposition which was offered to this theory, first by Hoffmann, and afterwards by Haller. The former of these had promulgated, as we have seen, the rival theory of a Nervous Fluid, the latter was the principal asserter of the doctrine of Irritability, an important theory on which we may afterwards have to touch. Haller's animosity against the Stahlian hypothesis is a remarkable feature in one who is in general so tolerant in his judgment of opinions. His arguments are taken from the absence of the control of the will over the vital actions, from the want of consciousness accompanying these actions, from the uniformity of them in different conditions of the mind, and from the small sensibility of

[55] Spr. v. 339, &c.
[56] *Ib.* 358.
[57] A.D. 1799.
[58] Spr. v. 360.

the heart which is the source of the vital actions. These objections, and the too decided distinction which Haller made between voluntary and involuntary muscles, were very satisfactorily answered by Whytt and Platner. In particular it was urged that the instinctive actions of brutes are inexplicable by means of mechanism, and may be compared with the necessary vital actions of the human body. Neither kind are accidental, neither kind are voluntary, both are performed without reflection.

Without tracing further the progress of the Psychical Doctrine, I shall borrow a few reflections upon it from Sprengel[59]:—

'When the opponents of the Stahlian system repeat incessantly that the assumption of a psychical cause in corporeal effects is a metaphysical speculation which does not belong to medicine, they talk to no purpose. The states of the soul are objects of our internal experience, and interest the physician too nearly to allow him to neglect them. The innumerable unconscious efforts of the soul, the powerful and daily effects of the passions upon the body, too often put to confusion those who would expel into the region of metaphysics the dispositions of the mind. The connexion of our knowledge of the soul, as gathered from experience, with our knowledge of the human body, is far closer than the mechanical and chemical physiologists suspect.

'The strongest objection against the psychical system, and one which has never been sufficiently answered by any of its advocates, is the universality of organic effects in the *vegetable* kingdom. The comparison of the physiology of plants with the physiology of animals puts the latter in its true light. Without absolutely trifling with the word *soul*, we cannot possibly derive from a soul the organic operations of vegetables. But just as little can we, as some Stahlians have done, draw a sharp line between plants and animals, and ascribe the processes of the former to mere mechanism, while

[59] Spr. v. 383.

we derive the operations of the latter from an intellectual principle. Not to mention that such a line is not possible, the rise of the sap and the alteration of the fluids of plants cannot be derived entirely from material causes as their highest origin.'

Thus, I may add, this psychical theory, however difficult to defend in its detail, does in its generalities express some important truths respecting the vital powers. It not only, like the last theory, gives unity to the living body, but it marks, more clearly than any other theory, the wide interval which separates mechanical and chemical from vital action, and fixes our attention upon the new powers which the consideration of life compels us to assume. It not only reminds us that these powers are elevated above the known laws of the material world, but also that they are closely connected with the world of thought and feeling, of will and reason; and thus it carries us, in a manner in which none of the preceding theories have done, to a true conception of a living, conscious, sentient, active individual.

At the same time we cannot but allow that the life of plants and of the lower orders of animals shows us very clearly that, in order to arrive at any sound and consistent knowledge respecting life, we must form some conception of it from which all the higher attributes which the term 'soul' involves, are utterly and carefully excluded; and therefore we cannot but come to the conclusion that the psychical school are right mainly in this; that in ascribing the functions of life to a *soul*, they mark strongly and justly the impossibility of ascribing them to any known attributes of *body*.

CHAPTER III.

ATTEMPTS TO ANALYSE THE IDEA OF LIFE.

1. *Definitions of Life.*—We have seen in the preceding chapter that all attempts to obtain a distinct conception of the nature of Life in general have ended in failure, and produced nothing beyond a negative result. And the conjecture may now naturally occur, that the cause of this failure resides in an erroneous mode of propounding to ourselves the problem. Instead of contemplating Life as a single Idea, it may perhaps be proper to separate it into several component notions: instead of seeking for one cause of all vital operations, it may be well to look at the separate vital functions, and to seek their causes. When the view of this possibility opens upon us, how shall we endeavour to verify it, and to take advantage of it?

Let us, as one obvious course, take some of the attempts which have been made to *define* Life, and let us see whether they appear to offer to us any analysis of the idea into component parts. Such definitions, when they proceed from men of philosophical minds, are the ultimate result of a long course of thought and observation; and by no means deserve to be slighted as arbitrary selections of conditions, or empty forms of words.

2. Life has been defined by Stahl[1], 'The condition by which a body resists a natural tendency to chemical changes, such as putrefaction.' In like manner, M. von Humboldt[2] defines living bodies to be 'those which, notwithstanding the constant operation

[1] Treviranus, *Biologie*, p. 19. Stahlii, *Their. Med.* p. 254.
[2] *Aphorismen aus d. Chem. Physiol. der Pflanzen*, s. 1.

of causes tending to change their form, are hindered by a certain inward power from undergoing such change.' The first of these definitions amounts only to the assertion, that vital processes are not chemical; a negative result, which we may accept as true, but which is, as we have seen, a barren truth. The second appears to be, in its import, identical with the first. An *inward* principle can only be understood as distinguished from known external powers, such as mechanical and chemical agencies. Or if, by an internal principle, we mean such a principle as that of which we are *conscious* within ourselves, we ascribe a soul to all living things: an hypothesis which we have seen is not more effective than the former in promoting the progress of biological science. Nearly the same criticism applies to such definitions as that of Kant: that 'Life is an internal faculty producing change, motion, and action.'

Other definitions refer us, not to some property residing in the whole of an organized mass, but to the connexion and relation of its parts. Thus M. von Humboldt[3] has given another definition of a living body: that 'it is a whole whose parts, arbitrarily separated, no longer resist chemical changes.' But this additional assertion concerning the parts, adds nothing of any value to the definition of the whole. And in some of the lower kinds of plants and animals it is hardly true as a fact.

3. Another definition[4] places the character of Life in 'motions serviceable to the body moved.' To this it has been objected[5], that, on this definition, the earth and the planets are living bodies. Perhaps it would be more philosophical to object to the introduction of so loose a notion as that of a property being *serviceable* to a body. We might also add, that if we speak of all vital functions as *motions*, we make an assumption quite unauthorized, and probably false.

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[3] *Versuche über die gereitzte Muskel und Nervenfäser*, b. ii. p. 433.

[4] Erhard, Röschlaub's *Magazin der Heilkunde*, b. i. st. 1. p. 69.

[5] Treviranus, *Biologie*, p. 41.

Other definitions refer the idea of Life to the idea of Organization. 'Life is the activity of matter according to laws of organization[6].' We are then naturally led to ask, What is Organization? In reply to this is given us the Kantian definition of Organization, which I have already quoted elsewhere[7], 'An organized product of nature is that in which all the parts are mutually ends and means[8].' That this definition involves exact fundamental ideas, and is capable of being made the basis of sound knowledge, I shall hereafter endeavour to show. But I may observe that such a definition leads us somewhat further. If the parts of organized bodies are known to be means to certain ends, this must be known because they fulfil these ends, and produce certain effects by the operation of a certain cause or causes. The question then recurs, what is *the cause* which produces such effects as take place in organized or living bodies? and this is identical with the problem of which in the last chapter we traced the history, and related the failure of physiologists in all attempts at its solution.

4. But what has been just said suggests to us that it may be an improvement to put our problem in another shape:—not to take for granted that the cause of all vital processes is one, but to suppose that there may be several separate causes at work in a living body. If this be so, life is no longer one kind of activity, but several. We have a number of operations which are somehow bound together, and life is the totality of all these: in short, life is not one Function, but a System of Functions.

5. We are thus brought very near to the celebrated definition of life given by Bichat[9]: 'Life is the sum of the functions by which death is resisted.' But upon the definition thus stated, we may venture to observe;—first, that the introduction of the notion of

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[6] Schmid, *Physiologie*, b. ii. p. 274.

[7] *Hist. Ind. Sc.* b. xvii. c. viii. s. 2.

[8] Kant, *Urtheilskraft*, p. 296.

[9] *Physiological Researches on Life and Death.*

death in order to define the notion of *life* appears to be unphilosophical. We may more naturally define death with reference to life, as the cessation of life; or at least we may consider life and death as correlative and interdependent notions. Again, the word 'sum,' used in the way in which it here occurs, appears to be likely to convey an erroneous conception, as if the functions here spoken of were simply added to each other, and connected by co-existence. It is plain that our idea of life involves more than this: the functions are all clearly connected, and mutually depend on each other; nutrition, circulation, locomotion, reproduction,—each has its influence upon all the others. These functions not merely co-exist, but exist with many mutual relations and connexions; they are continued so as to form, not merely a *sum*, but a *system*. And thus we are led to modify Bichat's definition, and to say that *Life is the system of vital functions.*

6. But it will be objected that by such a definition we explain nothing: the notion of *vital functions*, it may be said, involves the idea of *life*, and thus brings us round again to our starting-point. Or if not, at least it is as necessary to define Vital Functions as to define Life itself, so that we have made little progress in our task.

To this we reply, that if any one seeks, upon such subjects, some ultimate and independent definition from which he can, by mere reasoning, deduce a series of conclusions, he seeks that which cannot be found. In the Inductive Sciences, a Definition does not form the basis of reasoning, *but points out the course of investigation.* The definition must include words; and the meaning of these words must be sought in the progress and results of observations, as I have elsewhere said[10]. 'The meaning of words is to be sought in the progress of thought; the history of science is our dictionary; the steps of scientific induction are our definitions.' It will appear, I think, that it is more easy for us to form an idea of a separate Function of the

[10] *Hist. Ind. Sc.* b. xiii. c. ix.

animal frame, as Nutrition or Reproduction, than to comprehend Life in general under any single idea. And when we say that Life is a system of Vital Functions, we are of course directed to study these functions separately, and (as in all other subjects of scientific research) to endeavour to form of them such clear and definite ideas as may enable us to discover their laws.

7. The view to which we are thus led, of the most promising mode of conducting the researches of Biology, is one which the greatest and most philosophical physiologists of modern times have adopted. Thus Cuvier considers this as the true office of physiology at present. 'It belongs to modern times,' he says, 'to form a just classification of the vital phenomena; the task of the present time is to analyse the forces which belong to each organic element, and upon the zeal and activity which are given to this task, depends, according to my judgment, the fortune of physiology[11].' This classification of the phenomena of life involves, of course, a distinction and arrangement of the vital functions; and the investigation of the powers by which these functions are carried on, is a natural sequel to such a classification.

8. *Classifications of Functions.*—Attempts to classify the Vital Functions of man were made at an early period, and have been repeated in great number up to modern times. The task of classification is exposed to the same difficulties, and governed by the same conditions, in this as in other subjects. Here, as in the case of other things, there may be many classifications which are moderately good and natural, but there is only one which is the best and the true natural system. Here, as in other cases, one classification brings into view one set of relations; another, another; and each may be valuable for its special purpose. Here, as in other cases, the classes may be well constituted, though the boundary lines which divide them be somewhat indistinct, and the order doubtful. Here,

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[11] *Hist. Sc. Nat. dep.* 1789, i. 218.

as in other cases, we may have approached to the natural classification without having attained it; and here, as in other cases, to *define* our classes is the last and hardest of our problems.

9. The most ancient classification of the Functions of living things[12], is the division of them into *Vital, Natural*, and *Animal*. The *Vital* Functions are those which cannot be interrupted without loss of life, as *Circulation, Respiration*, and *Nervous Communication*. The *Natural* Functions are those which without the intervention of the will operate on their proper occasions to preserve the bodies of animals; they are *Digestion, Absorption, Nutrition;* to which was added *Generation*. The *Animal* Functions are those which involve perception and will, by which the animal is distinguished from the vegetable; they are *Sensibility, Locomotion*, and *Voice*.

The two great grounds of this division, the distinction of functions which operate continually, and those which operate occasionally; and again, the distinction of functions which involve sensation and voluntary motion from those which do not; are truly of fundamental importance, and gave a real value to this classification. It was, however, liable to obvious objections: namely, *First*, that the names of the classes were ill chosen; for all the functions are natural, all are vital: *Second*, that the lines of demarcation between the classes are indefinite and ambiguous; Respiration is a *vital* function, as being continually necessary to life; but it is also a *natural* function, since it occurs in the formation of the nutritive fluid, and an *animal* function, since it depends in part on the will. But these objections were not fatal, for a classification may be really sound and philosophical, though its boundary lines are vague, and its nomenclature ill selected. The division of the functions we have mentioned kept its ground long; or was employed with a subdivision of one class, so as to make them four; the *vital, natural, animal* and *sexual* functions.

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[12] *Dict. des Sciences Nat.* art. *Fonctions.*

10. I pass over many intermediate attempts to classify the functions, and proceed to that of Bichat as that which is, I believe, the one most generally assented to in modern times. The leading principle in the scheme of this celebrated physiologist is the distinction between *organic* and *animal* life. This separation is nearly identical with the one just noticed between the vital and animal functions; but Bichat, by the contrasts which he pointed out between these classes of functions, gave a decided prominence and permanence to the distinction. The Organic Life, which in animals is analogous to the life of vegetables, and the Animal Life, which implies sensation and voluntary motion, have each its system of organs. The center of the animal life is the brain, of the organic life, the heart. The former is carried on by a symmetrical, the latter, by an unsymmetrical system of organs: the former produces intermitting, the latter continuous actions: and, in addition to these, other differences are pointed out. This distinction of the two *lives*, being thus established, each is subdivided into two orders of Functions. The Animal Functions are passive, as *Sensation:* or active, as *Locomotion* and *Voice;* again, the Organic Functions are those of Composition, which are concerned in taking matter into the system; *Digestion*, *Absorption*, *Respiration*, *Circulation*, *Assimilation;* and those of Decomposition, which reject the materials when they have discharged their office in the system; and these are again, *Absorption*, *Circulation*, and *Secretion*. To these are added *Calorification*, or the production of animal heat. It appears, from what has been said, that *Absorption* and *Circulation* (and we may add *Assimilation* and *Secretion*, which are difficult to separate,) belong alike to the processes of composition and decomposition; nor in truth, can we, with any rigour, separate the centripetal and centrifugal movements in that vortex which, as we shall see, is an apt image of organic life.

Several objections have been made to this classification: and in particular, to the terms thus employed. It has been asserted to be a perversion of language to

ascribe to animals *two lives*, and to call the higher faculties in man, perception and volition, the *animal* functions. But, as we have already said, when a classification is really good, such objections, which bear only upon the mode in which it is presented, are by no means fatal: and it is generally acknowledged by all the most philosophical cultivators of biology, that this arrangement of the functions is better suited to the purposes of the science than those which preceded it.

11. But according to the principles which we have already laid down, the solidity of such a classification is to be verified by its serving as a useful guide in biological researches. If the arrangement which we have explained be really founded in natural relations, it will be found that in proportion as physiologists have studied the separate functions above enumerated, their ideas of these functions, and of the powers by which they are carried on, have become more and more clear; —have tended more and more to the character of exact and rigorous science.

To examine how far this has been the case with regard to all the separate functions, would be to attempt to estimate the value of all the principal physiological speculations of modern times; a task far too vast and too arduous for any one to undertake who has not devoted his life to such studies. But it may properly come within the compass of our present plan to show how, with regard to the broader lines of the above classification, there has been such a progress as we have above described, from more loose and inaccurate notions of some of the vital functions to more definite and precise ideas. This I shall attempt to point out in one or two instances.

CHAPTER IV.

ATTEMPTS TO FORM IDEAS OF SEPARATE VITAL FORCES, AND FIRST OF ASSIMILATION AND SECRETION.

SECT. I.—*Course of Biological Research.*

1. IT is to be observed that at present I do not speak of the progress of our knowledge with regard to the detail of the processes which take place in the human body, but of the approach made to some distinct Idea of the specially vital part of each process. In the History of Physiology, it has been seen[1] that all the great discoveries made respecting the organs and motions of the animal frame have been followed by speculations and hypotheses connected with such discoveries. The discovery of the circulation of the blood led to theories of animal heat; the discovery of the motion of the chyle led to theories of digestion; the close examination of the process of reproduction in plants and animals led to theories of generation. In all these cases, the discovery brought to light some portion of the process which was mechanical or chemical, but it also, in each instance, served to show that the process was something more than mechanical or chemical. The theory attempted to explain the process by the application of known causes; but there always remained some part of it which must unavoidably be referred to an unknown cause. But though unknown, such a cause was not a hopeless object of study. As the vital functions became better and better understood, it was seen more and more clearly at what precise points of the process it was necessary to assume a peculiar vital energy, and what sort of properties

[1] *Hist. Ind. Sc.* b. xvii.

this energy must be conceived to possess. It was perceived where, in what manner, in what degree, mechanical and chemical agencies were modified, over-ruled, or counteracted, by agencies which must be hypermechanical and hyperchemical. And thus the discoveries made in anatomy by a laborious examination of facts, pointed out the necessity of introducing new ideas, in order that the facts might be intelligible. Observation taught much; and among other things, she taught that there was something which could not be observed, but which must, if possible, be conceived. I shall notice a few instances of this.

SECT. II.—*Attempts to form a distinct Conception of Assimilation and Secretion.*

2. *The Ancients.*—That plants and animals grow by taking into their substance matter previously extraneous, is obvious to all: but as soon as we attempt to conceive this process distinctly in detail, we find that it involves no inconsiderable mystery. How does the same food become blood and flesh, bone and hair? Perhaps the earliest attempt to explain this mystery, is that recorded by Lucretius[2] as the opinion of Anaxagoras, that food contains some bony, some fleshy particles, some of blood, and so on. We might, on this supposition, conceive that the mechanism of the body appropriates each kind of particle to its suitable place.

But it is easy to refute this essay at philosophizing (as Lucretius refutes it) by remarking that we do not find milk in grass, or blood in fruit, though such food gives such products in cattle and in men. In opposition to this 'Homoiomereia,' the opinion that is forced upon us by the facts is, that the process of nutrition is not a selection merely, but an *assimilation;* the organized system does not *find*, but *make*, the additions to its structure.

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[2] Lucr. i. 855. Nunc et Anaxagoræ scrutemur ὁμοιομέρειαν.

3. *Buffon.*—This notion of *assimilation* may be variously expressed and illustrated; and all that we can do here, in order to show the progress of thought, is to adduce the speculations of those writers who have been most successful in seizing and marking its peculiar character. Buffon may be taken as an example of the philosophy of his time on this subject. 'The body of the animal,' says he[3], 'is a kind of *interior mould*, in which the matter subservient to its increase is modelled and assimilated to the whole, in such a way that, without occasioning any change in the order and proportion of the parts, there results an augmentation in each part taken separately. This increase, this development, if we would have a *clear idea* of it, how can we obtain it, except by considering the body of the animal, and each of the parts which is to be developed, as so many interior moulds which only receive the accessory matter in the order which results from the position of all their parts? This development cannot take place, as persons sometimes persuade themselves, by an addition to the outside; on the contrary, it goes on by an intimate susception which penetrates the mass; for, in the part thus developed, the size increases in all parts proportionally, so that the new matter must penetrate it in all its dimensions: and it is quite necessary that this penetration of substance must take place in a certain order, and according to a certain measure; for if this were not so, some parts would develope themselves more than others. Now what can there be which shall prescribe such a rule to the accessory matter except the *interior mould?*'

To speak of a *mould* simply, would convey a coarse mechanical notion, which could not be received as any useful contribution to physiological speculation. But this *interior* mould is, of course, to be understood figuratively, not as an assemblage of cavities, but as a collection of laws, shaping, directing, and modifying the new matter; giving it not only form, but motion

[3] *Hist. Nat.* b. i. c. iii.

and activity, such as belong to the parts of an organic being.

4. It must be allowed, however, that even with this explanation, the comparison is very loose and insufficient. A *mould* may be permitted to mean a collection of laws, but still it can convey no conception except that of laws regulated by relations of space; and such a conception is very plainly quite inadequate to the purpose. What can we conceive of the interior mould by which chyle is separated from the aliments at the pores of the lacteals, or tears secreted in the lacrymatory gland?

An additional objection to this mode of expression of Buffon is, that it suggests to us only a single marked change in the assimilated matter, not a continuous series of changes. Yet the animal fluids and other substances are, in fact, undergoing a constant series of changes. Food becomes chyme, and chyme becomes chyle; chyle is poured into the blood; from the blood secretions take place, as the bile; the bile is poured into the digestive canal, and a portion of the matter previously introduced is rejected out of the system. Here we must have a series of 'interior moulds;' and these must impress matter at its ejection from the organic system as well as at its reception. But, moreover, it is probable that none of the above transformations are quite abrupt. Change is going on between the beginning and the end of each stage of the nutritive circulation. To express the laws of this continuous change, the image of an interior mould is quite unsuited. We must seek a better mode of conception.

5. Vegetable and animal nutrition is, as we have said, a constant circulation. The matter so assumed is not all retained: a perpetual subtraction accompanies a perpetual addition. There is an excretion as well as an intussusception. The matter which is assumed by the living creature is retained only for a while, and is then parted with. The individual is the same, but its parts are in a perpetual flux: they come and go. For a time the matter which belongs to the organic body is bound to it by certain laws: but before it is thus bound, and

after it is loose, this matter may circulate about the universe in any other form. Life consists in a permanent influence over a perpetually changing set of particles.

Cuvier.—This condition also has been happily expressed, by means of a comparison, by another great naturalist. 'If,' says Cuvier[4], 'if, in order to obtain a just idea of the essence of life, we consider it in the beings where its effects are most simple, we shall soon perceive that it consists in the faculty which belongs to certain bodily combinations to continue during a determinate time under a determinate form; constantly attracting into their composition a part of the surrounding substances, and giving up in return some part of their own substance.

'Life is thus a *vortex*, more or less rapid, more or less complex, which has a constant direction, and which always carries along its stream particles of the same kinds; but in which the individual particles are constantly entering in and departing out; so that the *form* of the living body is more essential to it than its matter.

'So long as this motion subsists, the body in which it takes place is *alive;* it *lives.* When the motion stops finally, the body *dies.* After death, the elements which compose the body, given up to the ordinary chemical affinities, soon separate, and the body which was alive is dissolved.'

This notion of a vortex[5] which is permanent while the matter which composes it constantly changes,—of peculiar forces which act in this vortex so long as it exists, and which give place to chemical forces when

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[4] *Règne Animal,* i. 11.

[5] The definition of life given by M. de Blainville appears to me not to differ essentially from that of Cuvier: 'Un corps vivant est une sorte de foyer chimique où il-y-a à tous momens apport de nouvelles molecules et départ de molecules anciennes; où la composition n'est jamais fixe (si ce n'est d'un certain nombre de parties veritablement mortes ou en depôt), mais toujours pour ainsi dire *in nisu,* d'où mouvement plus ou moins lent et quelquefois chaleur.'—*Principes d'Anat.* 1822, t. i. p. 15.

the circulatory motion ceases,—appears to express some of the leading conditions of the assimilative power of living things in a simple and general manner, and thus tends to give distinctness to the notion of this vital function.

6. But we may observe that this notion of a Vortex is still insufficient. Particles are not only taken into the system and circulated through it for a time, but, as we have seen, they are altered in character in a manner to us unintelligible, both at their first admission into the system and at every period of their progress through it. In the vortex each particle is constantly *transformed* while it whirls.

It may be said, perhaps, that this transformation of the kinds of matter may be conceived to be merely a new arrangement of their particles, and that thus all the changes which take place in the circulating substances are merely so many additional windings in the course of the whirling current. But to say this, is to take for granted the atomic hypothesis in its rudest form. What right have we to assume that blood and tears, bile and milk, consist of like particles of matter differently arranged? What can arrangement, a mere relation of space, do towards explaining such differences? Is not the insufficiency, the absurdity of such an assumption proved by the whole course of science? Are not even chemical changes, according to the best views hitherto obtained, something more than a mere new arrangement of particles? And are not vital as much beyond chemical, as chemical are beyond geometrical modifications? It is not enough, then, to conceive life as a vortex. The particles which are taken into the organic frame do more than circulate there. They are, at every point of their circulation, acted upon by laws of an unknown kind, changing the nature of the substance which they compose. Life is a vortex in which vital forces act at every point of the stream: it is not only a current of whirling *matter*, but a cycle of recurring *powers*.

7. *Matter and Form.*—This image of a vortex is closely connected with the representation of life offered

us by writers of a very different school. In Schelling's *Lectures on Academic Study*, he takes a survey of the various branches of human knowledge, determining according to his own principles the shape which each science must necessarily assume. The peculiar character of organization, according to him[6], is that the matter is only an accident of the thing itself, and the organization consists in Form alone. But this Form, by its very opposition to Matter, ceases to be independent of it, and is only ideally separable. In organization, therefore, substance and accident, matter and form, are completely identical[7]. This notion, that in organization the Form is essential and the Matter accidental, or, in other words, that the Form is permanent and the Matter fluctuating and transitory, agrees, if taken in the grossest sense of matter and form, with Cuvier's image of a Vortex. In a whirlpool, or in a waterfall, the form remains, the matter constantly passes away and is renewed. But we have already seen[8] that in metaphysical speculations in which *matter* and *form* are opposed, the word *form* is used in a far more extensive sense than that which denotes a relation of space. It may indeed designate any change which matter can undergo; and we may very allowably say that food and blood are the same matter under different *forms*. Hence if we assert that *Life is a constant Form of a circulating Matter*, we express Cuvier's notion in a mode free from the false suggestion which 'Vortex' conveys.

8. We may, however, still add something to this account of life. The circulating parts of the system not only circulate, but they form the non-circulating parts. Or rather, there are no non-circulating parts: all portions of the frame circulate more or less rapidly. The food which we take circulates rapidly in the fluids, more slowly in the flesh, still more slowly in the bones; but in all these parts it is taken into the system,

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[6] Lect. xiii. p. 288.

[7] I have not translated Schelling's words, but given their import as far as I could.

[8] Book i.

retained there for some time, and finally replaced by other matter. But while it remains in the body, it exercises upon the other circulating parts the powers by which their motion is produced. Nutriment forms and supports the organs, and the organs carry fresh nutriment to its destination. The peculiar forces of the living body, and its peculiar structure, are thus connected in an indescribable manner. The forces produce the structure; the structure, again, is requisite for the exertion of the forces. The Idea of an Organic or Living Being includes this peculiar condition—that its construction and powers are such, that it constantly appropriates to itself new portions of substance which, so appropriated, become indistinguishable parts of the whole, and serve to carry on subsequently the same functions by which they were assimilated. And thus *Organic Life is a constant Form of a circulating Matter, in which the Matter and the Form determine each other by peculiar laws* (*that is, by Vital Forces*).

SECT. III.—*Attempts to conceive the forces of Assimilation and Secretion.*

9. I have already stated that in our attempts to obtain clear and scientific Ideas of Vital Forces, we have, in the first place, to seek to understand the course of change and motion in each function, so as to see at what points of the process peculiar causes come into play; and next, to endeavour to obtain some insight into the peculiar character and attributes of these causes. Having spoken of the first part of this mode of investigation in regard to the general nutrition of organic bodies, I must now say a few words on the second part.

The Forces here spoken of are *Vital* Forces. From what has been said, we may see in some measure the distinction between forces of this kind and mechanical or chemical forces; the latter tend constantly to produce a final condition, after which there is no further cause of change: mechanical forces tend to produce equilibrium; chemical forces tend to produce composi-

tion or decomposition; and this point once reached, the matter in which these forces reside is altogether inert. But an organic body tends to a constant motion, and the highest activity of organic forces shows itself in continuous change. Again, in mechanical and chemical forces, the force of any aggregate is the sum of the forces of all the parts: the sum of the forces corresponds to the sum of the matter. But in organic bodies, the amount of effect does not depend on the matter, but on the form: the particles lose their separate energy, in order to share in that of the system; they are not added, they are *assimilated.*

10. It is difficult to say whether anything has been gained to science by the various attempts to assign a fixed *name* to the vital force which is thus the immediate cause of Assimilation. It has been called *Organic Attraction* or *Vital Attraction*, *Organic Affinity* or *Vital Affinity*, being thus compared with mechanical Attraction or chemical Affinity. But, perhaps, as the process is certainly neither mechanical nor chemical, it is desirable to appropriate to it a peculiar name; and the name *Assimilation*, or *Organic Assimilation*, by the usage of good biological writers, is generally employed for this purpose, and may be taken as the standard name of this Vital Force. To illustrate this, I will quote a passage from the excellent *Elements of Physiology* of Professor Müller. 'In the process of nutrition is exemplified the fundamental principle of *organic assimilation.* Each elementary particle of an organ attracts similar particles from the blood, and by the changes it produces in them, causes them to participate in the vital principle of the organ itself. Nerves take up nervous substance, muscles, muscular substance: even morbid structures have the assimilating power; warts in the skin grow with their own peculiar structure; in an ulcer, the base and border are nourished in a way conformable to the mode of action and secretion determined by the disease.'

11. The Force of Organic Assimilation spoken of in the last paragraph denotes peculiarly the force by which each organ appropriates to itself a part of the

nutriment received into the system, and thus is maintained and augmented with the growth of the whole. But the growth of the solid parts is only one portion of the function of nutrition; besides this, we must consider the motion and changes of the fluids, and must ask what kind of forces may be conceived to produce these. What are the powers by which chyle is *absorbed* from the food, by which bile is *secreted* from the blood, by which the circulating *motion* of these and all other fluids of the body are constantly maintained? To the questions,—What are the forces by which *absorption*, *secretion*, and the *vital motions*, of fluids are produced?—no satisfactory answer has been returned. Yet still some steps have been made, which it may be instructive to point out.

12. In *Absorption* it would appear that a part of the agency is inorganic; for not only dead membranes, but inorganic substances, absorb fluids, and even absorb them with elective forces, according to the ingredients of the fluid. A force which is of this kind, and which has been termed *Endosmose*, has been found to produce very curious effects. When a membrane separates two fluids, holding in solution different ingredients, the fluids pass through the membrane in an imperceptible manner, and mix or exchange their elements. The force which produces these effects is capable of balancing a very considerable pressure. It appears, moreover, to depend, at least among other causes, upon attractions operating between the elements of the solids and the fluids, as well as between the different fluids; and this force, though thus apparently of a mechanical and chemical nature, probably has considerable influence in vital phenomena.

13. But still, though Endosmose may account in part for absorption in some cases, it is certain that there is some other vital force at work in this process. There must be, as Müller says[9], 'an organic attraction of a kind hitherto unknown.' 'If absorption,' he adds[10], is to be explained in a manner analogous to

[9] *Physiology*, p. 299.

[10] *Ib.* p. 301.

the laws of endosmose, it must be supposed that a chemical affinity, resulting from the vital process itself, is exerted between the chyme in the intestines and the chyle in the lacteals, by which the chyle is enabled to attract the chyme without being itself attracted by it. But such affinity or attraction would be of a *vital* nature, since it does not exist after death.'

14. If the force of absorption be thus mysterious in its nature, the force of *Secretion* is still more so. In this case we have an organ filled with a fine net-work of blood-vessels, and in the cavities of some *gland*, or open part, we have a new fluid formed, of a kind altogether different from the blood itself. It is easily shown that this cannot be explained by any action of pores or capillary tubes. But what conception can we form of the forces by which such a change is produced? Here, again, I shall borrow the expressions of Müller, as presenting the last result of modern physiology. He says[11], 'The more probable supposition is, that by virtue of imbibition, or the general organic porosity, the fluid portion of the blood becomes diffused through the tissue of the secreting organ; that the external surface of the glandular canals exerts a chemical attraction on the elements of the fluid, infusing into them at the same time a tendency to unite in new combinations; and then repels them in a manner which is certainly quite inexplicable, towards the inner surface of the secreting membrane, or glandular canals.' 'Although quite unsupported by facts,' he adds, 'this theory of attraction and repulsion is not without its analogy in physical phenomena; and it would appear that very similar powers effect the elimination of the fluid in secretion, and cause it to be taken up by the lymphatics in absorption.' He elsewhere says[12], 'Absorption seems to depend on an attraction the nature of which is unknown, but of which the very counterpart, as it were, takes place in secretion; the fluids altered by the secreting action being repelled towards the free side or open surface only of the

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11 *Physiology*, p. 464.

12 *Ib.* p. 301.

secreting membranes, and then pressed forwards by the successive portions of the fluids secreted.'

15. With regard to the forces which produce the *Motion* of absorbed or secreted fluids along their destined course, it may be seen, from the last quoted sentence, that the same vital force which changes the nature, also produces the movement of the substance. The fluids are pressed forwards by the successive portions absorbed or secreted. That this is the sole cause, or at least a very powerful cause, of the motion of the nutritive fluids in organic bodies, is easily shown by experience. It is found[13] that the organs which effect the ascent of the sap in trees during the spring are the terminal parts of the roots; that the whole force by which the sap is impelled upwards is the *vis a tergo*, as it has been called, the force pushing from behind, exerted in the roots. And thus the force which produces this motion is exerted exactly at those points where the organic body selects from the contiguous mass those particles which it absorbs and appropriates. And the same may most probably be taken for the cause of the motion of the lymph and chyle; at least, Müller says[14] that no other motive power has been detected which impels those fluids in their course.

Thus, though we must confess the Vital Force concerned in Assimilation and Secretion to be unknown in its nature, we still obtain a view of some of the attributes which it involves. It has mechanical efficacy, producing motions, often such as would require great mechanical force. But it exerts at the same point both an attraction and a repulsion, attracting matter on one side, and repelling it on the other; and in this circumstance it differs entirely from mechanical forces. Again, it is not only mechanical but chemical, producing a complete change in the nature of the substance on which it acts; to which we must add that the changes produced by the vital forces are such as, for the most part, our artificial chemistry can-

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[13] Müller, p. 300.

[14] *Ib.* p. 254.

not imitate. But, again, by the action of the vital force at any point of an organ, not only are fluids made to pass, and changed as they pass, but the organ itself is maintained and strengthened, so as to continue or to increase its operation: and thus the vital energy supports its activity by its action, and is augmented by being exerted.

We have thus endeavoured to obtain a view of some of the peculiar characters which belong to the Force of Organic Assimilation;—the Force by which life is kept up, conceived in the most elementary form to which we can reduce it by observation and contemplation. It appears that it is a force which not only produces motion and chemical change, but also *vitalizes* the matter on which it acts, giving to it the power of producing like changes on other matter, and so on indefinitely. It not only circulates the particles of matter, but puts them in a stream of which the flow is development as well as movement.

The force of Organic Assimilation being thus conceived, it becomes instructive to compare it with the force concerned in Generation, which we shall therefore endeavour to do.

SECT. IV.—*Attempts to conceive the Process of Generation.*

16. At first sight the function of Nutrition appears very different from the function of Generation. In the former case we have merely the existing organs maintained or enlarged, and their action continued; in the latter, we have a new individual produced and extricated from the parent. The term *Reproduction* has, no doubt, been applied, by different writers, to both these functions;—to the processes by which an organ when mutilated, is restored by the forces of the living body, and to the process by which a new generation of individuals is produced which may be considered as taking the place of the old generation, as these are gradually removed by death. But these are obviously different senses of the word. In the latter case, the

term *Reproduction* is figuratively used; for the *same* individuals are not reproduced; but the species is kept up by the propagation of new individuals, as in nutrition the organ is kept up by the assimilation of new matter. To escape ambiguity, I shall avoid using the term *Reproduction* in the sense of *Propagation*.

17. In Nutrition, as we have seen, the matter, which from being at first extraneous, is appropriated by the living system, and directed to the sustentation of the organs, undergoes a series of changes of which the detail eludes our observation and apprehension. The nutriment which we receive contributes to the growth of flesh and bone, viscera and organs of sense. But we cannot trace in its gradual changes a visible preparation for its final office. The portion of matter which is destined to repair the waste of the eye or the skin, is not found assuming a likeness to the parts of the eye or the structure of the skin, as it comes near the place where it is moulded into its ultimate form. The new parts are insinuated among the old ones, in an obscure and imperceptible matter. We can trace their progress only by their effects. The organs *are* nourished, and that is almost all we can learn: we cannot discover *how* this is done. We cannot follow nature through a series of manifest preparations and processes to this result.

18. In Generation the case is quite different. The young being is formed gradually and by a series of distinguishable processes. It is included within the parent before it is extruded, and approaches more or less to the likeness of the parent before it is detached. While it is still an embryo, it shares in the nutriment which circulates through the system of the mother; but its destination is already clear. While the new and the old parts, in every other portion of the mother, are undistinguishably mixed together, this new part, the fœtus, is clearly distinct from the rest of the system, and becomes rapidly more and more so, as the time goes on. And thus there is formed, not a new part, but a new whole; it is not an organ which is kept up, but an offspring which is prepared. The progeny is

included in the parent, and is gradually fitted to be separated from it. The young is at first only the development of a part of the organization of the mother;—of a germ, an ovule. But it is not developed like other organs, retaining its general form. It does not become merely a larger bud, a larger ovule; it is entirely changed; it becomes—from a bud—a blossom, a flower, a fruit, a seed; from an ovule it becomes an egg, a chick, a bird; or it may be, a fœtus, a child. The original rudiment is not merely nourished, but unfolded and transformed through the most marked and remote changes, gradually tending to the form of the new individual.

19. But this is not all. The fœtus is, as we have said, a development of a portion of the mother's organization. But the fœtus (supposing it female) is a likeness of the mother. The mother, even before conception, contains within herself the germs of her progeny; the female fœtus, therefore, at a certain stage of development, will contain also the germs of possible progeny; and thus we may have the germs of future generations, pre-existing and included successively within one another. And this state of things, which thus suggests itself to us as possible, is found to be the case in facts which observation supplies. Anatomists have traced ovules in the unborn fœtus, and thus we have three generations included one within another.

20. Supposing we were to stop here, the process of propagation might appear to be altogether different from that of nutrition. The latter, as we have seen, may be in some measure illustrated by the image of a *vortex;* the former has been represented by the image of a series of germs, *sheathed* one within another successively, and this without any limit. This view of the subject has been termed the doctrine of the *Pre-existence of germs;* and has been designated by German writers by a term 'Einschachtelungs-theorie' descriptive of the successive sheathing of which I have spoken. Imitating this term, we may call it *the Theory of successive inclusion.* It has always had many

adherents; and has been, perhaps, up to the present time, the most current opinion on the subject of generation. Cuvier inclines to this opinion[15]. 'Fixed forms perpetuating themselves by generation distinguish the species of living things. These forms do not produce themselves, do not change themselves. Life supposes them to exist already; its flame can be lighted only in organization previously prepared; and the most profound meditations and the most delicate researches terminate alike in the mystery of the *pre-existence of germs*.'

21. Yet this doctrine is full of difficulty. It is, as Cuvier says, a mysterious view of the subject;—so mysterious, that it can hardly be accepted by us, who seek distinct conceptions as the basis of our philosophy. Can it be true, not only that the germ of the offspring is originally included in the parent, but also the germs of *its* progeny, and so on without limit:—so that each fruitful individual contains in itself an infinite collection of future possible individuals;—a reserve of infinite succeeding generations? This is hard to admit. Have we no alternative? What is the opposite doctrine?

22. The opposite doctrine deserves at least some notice. It extends, to the production of a new individual, the conception of growth by nutrition. According to this view, we suppose propagation to take place, not as in the view just spoken of, by inclusion and extrusion, but by assimilation and development;—not by the material pre-existence of germs, but by the communication of vital forces to new matter. This opinion appears to be entertained by some of the most eminent physiologists of the present time. Thus, Müller says, 'The organic force is also creative. The organic force which resides in the whole, and on which the existence of each part depends, has also the property of generating, from organic matter, the parts necessary to the whole.' Life, he adds, is not merely a harmony of the

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[15] *Règne Animal*, p. 20.

parts. On the contrary, the harmonious action of the parts subsists only by the influence of a force pervading all parts of the body. 'This force exists before the harmonizing parts, which are in fact formed by it during the development of the embryo.' And again; 'The creative force exists in the germ, and creates in it the essential force of the future animal. The germ is *potentially* the whole animal: during the development of the germ the parts which constitute the actual whole are produced.'

23. In this view, we extend to the reproduction of an individual the same conception of organic assimilation which we have already arrived at, as the best notion we can form of the force by which the reproduction and sustentation of parts takes place. And is not such an extension really very consistent? If a living thing can appropriate to itself extraneous matter, invest it with its own functions, and thus put it in the stream of constant development, may we not conceive the development of a new *whole* to take place in this way as well as of a *part?* If the organized being can infuse into new matter its vital forces, is there any contradiction in supposing this infusion to take place in the full measure which is requisite for the production of a new individual? The force of organic assimilation is transferred to the very matter on which it acts; it may be transferred so that the operation of the forces produces not only an organ, but a system of organs.

24. This identification of the forces which operate in Nutrition and Generation may at first seem forced and obscure, in consequence of the very strong apparent differences of the two processes which we have already noticed. But this defect in the doctrine is remedied by the consideration of what may be considered as intermediate cases. It is not true that, in the nutrition of special organs, the matter is always conveyed to its ultimate destination without being on its way moulded into the form which it is finally to bear, as the embryo is moulded into the form of the

future individual. On the contrary, there are cases in which the waste of the organs is supplied by the growth of new ones, which are prepared and formed before they are used, just as the offspring is prepared and formed before it is separated from the parent. This is the case with the teeth of many animals, and especially with the teeth of animals of the crocodile kind. Young teeth grow near the root of the old ones, like buds on the stem of a plant; and as these become fully developed, they take the place of the parent tooth when that dies and is cast away. And these new teeth in their turn are succeeded by others which germinate from them. Several generations of such teeth, it is said as many as four, have been detected by anatomists, visibly existing at the same time; just as several generations of germs of individuals have been, as we already stated, observed included in one another. But this case of the teeth appears to show very strikingly how insufficient such observations are to establish the doctrine of successive inclusion, or of the pre-existence of germs. Are we to suppose that every crocodile's tooth includes in itself the germs of an infinite number of possible teeth, as in the theory of pre-existing germs every individual includes an infinite number of individuals? If this be true of teeth, we must suppose that organ to follow laws entirely different from almost every other organ; for no one would apply to the other organs in general such a theory of reproduction. But if such a theory be not maintained respecting the teeth, how can we maintain the theory of the pre-existing germs of individuals, which has no recommendation except that of accounting for exactly the same phenomena?

It would seem, then, that we are, by the closest consideration of the subject, led to conceive the forces by which generation is produced, as forces which vitalize certain portions of matter, and thus prepare them for development according to organic forms; and thus the conception of this Generative Force is identified with the conception of the Force of Organic Assimilation, to

which we were led by the consideration of the process of nutrition.

I shall not attempt to give further distinctness and fixity to this conception of one of the vital forces; but I shall proceed to exemplify the same analysis of life by some remarks upon another Vital Process, and the Forces of which it exhibits the operation.

CHAPTER V.

Attempts to form Ideas of separate Vital Forces, *continued.*—Voluntary Motion.

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1. WE formerly noticed the distinctions of *organic* and *animal* functions, organic and animal forces, as one of the most marked distinctions to which physiologists have been led in their analysis of the vital powers. I have now taken one of the former, the *organic* class of functions, namely, Nutrition; and have endeavoured to point out in some measure the peculiar nature of the vital forces by which this function is carried on. It may serve to show the extent and the difficulty of this subject, if, before quitting it, I offer a few remarks suggested by a function belonging to the other class, the *animal* functions. This I shall briefly do with respect to *Voluntary Motion.*

2. In the History of Physiology, I have already related the progress of the researches by which the organs employed in voluntary motion became known to anatomists. It was ascertained to the satisfaction of all physiologists, that the immediate agents in such motion are the muscles; that the muscles are in some way contracted, when the nerves convey to them the agency of the will; and that thus the limbs are moved. It was ascertained, also, that the nerves convey sensations from the organs of sense inwards, so as to make these sensations the object of the animal's consciousness. In man and the higher animals, these impressions upon the nerves are all conveyed to one internal organ, the brain; and from this organ all impressions of the will appear to proceed; and thus the brain is

the center of animal life, towards which sensations converge, and from which volitions diverge.

But this being the process, we are led to inquire how far we can obtain any knowledge, or form any conception, of the vital forces by means of which the process is carried on. And here I have further stated in the History[1], that the transfer of sensations and volitions along the nerves was often represented as consisting in the motion of a Nervous Fluid. I have related that the hypothesis of such a fluid, conveying its impressions either by motions of translation or of vibration, was countenanced by many great names, as Newton, Haller, and even Cuvier. But I have ventured to express my doubt whether this hypothesis can have much value: 'for,' I have said, 'this principle cannot be mechanical, chemical, or physical, and therefore cannot be better understood by embodying it in a fluid. The difficulty we have in conceiving what the force *is*, is not got rid of by explaining the machinery by which it is *transferred*.'

3. I may add, that no succeeding biological researches appear to have diminished the force of these considerations. In modern times, attempts have repeatedly been made to identify the nervous fluid with electricity or galvanism. But these attempts have not been satisfactory or conclusive of the truth of such an identity: and Professor Müller probably speaks the judgment of the most judicious physiologists, when he states it as his opinion, after examining the evidence[2], 'That the vital actions of the nerves are not attended with the development of any galvanic currents which our instruments can detect; and that the laws of action of the nervous principle are totally different from those of electricity.'

That the powers by which the nerves are the instruments of sensation, and the muscles of motion, are vital endowments, incapable of being expressed or explained by any comparison with mechanical, chemical, and electrical forces, is the result which we should

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[1] *Hist. Ind. Sc.* b. xvii. c. v. s. 2.

[2] *Elem. Phys.* p. 640.

expect to find, judging from the whole analogy of science; and which thus is confirmed by the history of physiology up to the present time. We naturally, then, turn to inquire whether such peculiar vital powers have been brought into view with any distinctness and clearness.

4. The property by which muscles, under proper stimulation, contract and produce motion, has been termed *Irritability* or *Contractility;* the property by which nerves are susceptible of their appropriate impressions has been termed *Sensibility.* A very few words on each of these subjects must suffice.

Irritability.—I have, in the History of Physiology[3], noticed that Glisson, a Cambridge professor, distinguished the Irritation of muscles as a peculiar property, different from any merely mechanical or physical action. I have mentioned, also, that he divides Irritation into *natural, vital,* and *animal;* and points out, though briefly, the graduated differences of Irritability in different organs. Although these opinions did not at first attract much notice, about seventy years afterwards attention was powerfully called to this vital force, *Irritability,* by Haller. I shall borrow Sprengel's reflections on this subject.

'Hitherto men had been led to see more and more clearly that the cause of the bodily functions, the fundamental power of the animal frame, is not to be sought in the mechanism, and still less in the mixture of the parts. In this conviction, they had had recourse partly to the quite supersensuous principle of the Soul, partly to the half-material principle of the Animal Spirits, in order to explain the bodily motions. Glisson alone saw the necessity of assuming an Original Power in the fibres, which, independent of the influence of the animal spirits, should produce contraction in them. And Gorter first held that this Original Power was not to be confined to the muscles, but to be extended to all parts of the living body.

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[3] *Hist. Ind. Sc.* b. xvii. c. v.

'But as yet the laws of this Power were not known, nor had men come to an understanding whether it were fully distinct from the elasticity of the parts, or by what causes it was put in action. They had neither instituted observations nor experiments which established its relation to other assumed forces of the body. There was still wanting a determination of the peculiar seat of this power, and experiments to trace its gradual differences in different parts of the body. In addition to other causes, the necessity of the assumption of such a power was felt the more, in consequence of the prevalence of Leibnitz's doctrine of the activity of matter; but it was an occult quality, and remained so till Haller, by numerous experiments and solid observations, placed in a clear light the peculiarities of the powers of the animal body.'

5. Perhaps, however, Haller did more in the way of determining experimentally the limits and details of the application of this idea of Irritability as a peculiar attribute, than in developing the Idea itself. In that way his merits were great. As early as the year 1739, he published his opinion upon Irritability as the cause of muscular motion, which he promulgated again in 1743. But from the year 1747 he was more attentive to the peculiarities of Irritability, and its difference from the effect of the nerves. In the first edition of his *Physiology*, which appeared in 1747, he distinguished three kinds of Force in muscles,—the Dead Force, the Innate Force, and the Nervous Power. The first is identical with the elastic force of dead matter, and remains even after death. The *innate force* continues only a short time after death, and discloses itself especially by alternate oscillations; the motions which arise from this are much more lively than those which arise from mere elasticity: they are not excited by tension, nor by pressure, nor by any mechanical alteration, but only by *irritation*. The *nervous force* of the muscle is imparted to it from without by the nerves; it preserves the *irritability*, which cannot long subsist without the influence of the nervous force, but is not identical with it.

In the year 1752, Haller laid before the Society of Göttingen the result of one hundred and ninety experiments; from which it appears to what parts of the animal system Irritability and Nervous Power belong. These I need not enumerate. He also investigated with care its gradations in those parts which do possess it. Thus the heart possesses it in the highest degree, and other organs follow in their order.

6. Haller's doctrine was, that there resides in the muscles a peculiar vital power by which they contract, and that this power is distinct from the attributes of the nerves. And this doctrine has been accepted by the best physiologists of modern times. But this distinction of the *irritability* of the muscles from the *sensibility* of the nerves became somewhat clearer by giving to the former attribute the name of *Contractility*. This accordingly was done; it is, for example, the phraseology used by Bichat. By speaking of *animal sensibility* and *animal contractility*, the passive and the active element of the processes of animal life are clearly separated and opposed to each other. The sensations which we feel, and the muscular action which we exert, may be closely and inseparably connected, yet still they are clearly distinguishable. We can easily in our apprehension separate the titillation felt in the nose on taking snuff, from the action of the muscles in sneezing; or the perception of an object falling towards the eye, from the exertion which shuts the eye-lid; although in these cases the passive and active part of the process are almost or quite inseparable in fact. And this clear separation of the active from the passive power is something, it would seem, peculiar to the Animal Vital Powers; it is a character by which they differ, not only from mechanical, chemical, and all other merely physical forces, but even from Organic Vital Powers.

7. But this difference between the Animal and the Organic Vital Powers requires to be further insisted upon, for it appears to have been overlooked or denied by very eminent physiologists. For instance, Bichat classifies the Vital Powers as Animal Sensibility, Ani-

mal Contractility, Organic Sensibility, Organic Contractility.

Now the view which suggests itself to us, in agreement with what has been said, is this:—that though Animal Sensibility and Animal Contractility are clearly and certainly distinct, Organic Sensibility and Organic Contractility are neither separable in fact nor in our conception, but together make up a single Vital Power. That they are not separable in fact is, indeed, acknowledged by Bichat himself. 'The organic contractility,' he says[4], 'can never be separated from the sensibility of the same kind; the reaction of the excreting tubes is immediately connected with the action which the secreted fluids exercise upon them: the contraction of the heart must necessarily succeed the influx of the blood into it.' It is not wonderful, therefore, that it should have happened, as he complains, that 'authors have by no means separated these two things, either in their consideration or in language.' We cannot avoid asking, Are Organic Sensibility and Organic Contractility really anything more than two different aspects of the same thing, like action and reaction in mechanics, which are only two ways of considering the action which takes place at a point; or like the positive and negative electricities, which, as we have seen, always co-exist and correspond to each other?

8. But we may observe, moreover, that Bichat, by his use of the term Contractility, includes in it powers to which it cannot with any propriety be applied. Why should we suppose that the vital powers of absorption, secretion, assimilation, are of such a nature that the name *contractility* may be employed to describe them? We have seen, in the last chapter, that the most careful study of these powers leads us to conceive them in a manner altogether removed from any notion of contraction. Is it not then an abuse of language which cannot possibly lead to anything but

[4] *Life and Death*, p. 94.

confusion, to write thus[5]: 'The insensible organic contractility is that, by virtue of which the excreting tubes react upon their respective fluids, the secreting organs upon the blood which flows into them, the parts where nutrition is performed upon the nutritive juices, and the lymphatics upon the substances which excite their open extremities'? In the same manner he ascribes[6] to the peculiar sensibility of each organ the peculiarity of its products and operations. An increased absorption is produced by an increased susceptibility of the 'absorbent orifices.' And thus, in this view, each organic power may be contemplated either as sensibility or as contractility, and may be supposed to be rendered more intense by magnifying either of these its aspects; although, in fact, neither can be conceived to be increased without an exactly commensurate increase of the other.

9. This opinion, unfounded as it thus appears to be, that all the different organic vital powers are merely different kinds of Contractility or Excitability, was connected with the doctrines of Brown and his followers, which were so celebrated in the last century, that all diseases arise from increase or from diminution of the Vital Force. The considerations which have already offered themselves would lead us to assent to the judgment which Cuvier has pronounced upon this system. 'The theory of excitation,' he says, 'so celebrated in these later times by its influence upon pathology and therapeutick, is at bottom only a modification of that, in which, including under a common name Sensibility and Irritability,' and we may add, applying this name to all the Vital Powers, 'the speculator takes refuge in an abstraction so wide, that if, by it, he simplifies medicine, he by it annihilates all positive physiology[7].'

10. The separation of the nervous influence and the muscular irritability, although it has led to many highly instructive speculations, is not without its diffi-

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[5] *Life and Death*, p. 95. [6] *Ib.* p. 90.

[7] *Hist. des Sc. Nat. depuis* 1789, i. 219.

culties, when viewed with reference to the Idea of Vital Power. If the irritability of each muscle reside in the muscle itself, how does it differ from a mere mechanical force, as elasticity? But, in point of fact, it is certain that the muscular irritability of the animal body is not an attribute of the muscle itself independent of its connexion with the system. No muscle, or other part, removed from the body, *long* preserves its irritability. This power cannot subsist permanently, except in connexion with an organic whole. This condition peculiarly constitutes irritability a *living* force: and this condition would be satisfied by considering the force as derived from the nervous system; but it appears that though the nervous system has the most important influence upon all vital actions, the muscular irritability must needs be considered as something distinct. And thus the Irritability or Contractility of the muscle is a peculiar endowment of the texture, but it is at the same time an endowment which can only co-exist with life; it is, in short, a peculiar Vital Power.

11. This necessity of the union of the muscle with the whole nervous system, in order that it may possess irritability, was the meaning of the true part of Stahl's psychical doctrine; and the reason why he and his adherents persisted in asserting the power of the soul even over involuntary motions. This doctrine was the source of much controversy in later times.

'But,' says Cuvier[8], 'this opposition of opinion may be reconciled by the intimate union of the nervous substance with the fibre and the other contractile organic elements, and by their reciprocal action;—doctrines which had been presented with so much probability by physiologists of the Scotch school, but which were elevated above the rank of hypotheses only by the observations of more recent times.

'The fibre does not contract by itself, but by the influence of the nervous filaments, which are always united with it. The change which produces the con-

[8] *Hist. des Sc. Nat. depuis* 1789, i. 213.

traction cannot take place without the concurrence of both these substances; and it is further necessary that it should be occasioned each time by an exterior cause, by a stimulant.

'The Will is one of these stimulants; but it only excites the Irritability, it does not constitute it; for in the case of persons paralytic from apoplexy, the Irritability remains, though the power of the Will over it is gone. Thus *irritability* depends in part on the *nerve*, but not on the *sensibility:* this last is another property, still more admirable and occult than the irritability; but it is only one among several functions of the nervous system. It would be an abuse of words to extend this denomination to functions unaccompanied by perception.'

12. Supposing, then, that Contractility is established as a peculiar Vital Power residing in the muscles, we may ask whether we can trace with any further exactness the seat and nature of this power. It would be unsuitable to the nature of the present work to dwell upon the anatomical discussions bearing upon this point. I will only remark that some anatomists maintain[9] that muscles are contracted by those fibres assuming a zigzag form, which at first were straight. Others (Professor Owen and Dr. A. Thompson) doubt the accuracy of this observation; and conceive that the muscular fibre becomes shorter and thicker, but does not deviate from a right line. We may remark that the latter kind of action appears to be more elementary in its nature. We can, as a matter of geometry, conceive a straight line thrown into a zigzag shape by muscular contractions taking place between remote parts of it; but it is difficult to conceive by what *elementary* mode of action a straight fibre could bend itself at certain points, and at certain points only; since the elementary force must act at every point of the fibre, and not at certain selected points.

13. A circumstance which remarkably marks the difference between the vital force of Contractility, in-

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[9] Müller, *Elem. Phys.* p. 887.

herent in muscles, and any merely dead or mechanical force, is this; that in assuming their contractile state, muscles exert a tension which they could not themselves support or convey if not strengthened by their vital irritability. They are capable of raising weights by their exertion, which will tear them asunder when the power of contraction is lost by death. This has induced Cuvier and other physiologists[10] to believe 'that in the moment of action, the particles that compose a fibre, not only approach towards each other longitudinally, but that their cohesive attraction becomes instantaneously much greater than it was before: for without such an increase of cohesive force, the tendency to shorten could not, as it would appear, prevent the fibre from being torn.' We see here the difficulty, or rather the impossibility, of conceiving muscular contractility as a mere mechanical force; and perhaps there is little hope of any advantage by calling in the aid of chemical hypothesis to solve the mechanical difficulty. Cuvier conjectures that a sudden change in the chemical composition may thus so quickly and powerfully augment the cohesion. But we may ask, are not a chemical synthesis and analysis, suddenly performed by a mere act of the will, as difficult to conceive as a sudden increase and decrease of mechanical power directly produced by the same cause?

14. *Sensibility.* The nerves are the organs and channels of Sensibility. By means of them we receive our sensations, whether of mere pleasure and pain, or of qualities which we ascribe to external objects, as a bitter taste, a sweet odour, a shrill sound, a red colour, a hard or a hot feeling of touch. Some of these sensations are but obscurely the objects of our consciousness; as for example the feeling which our feet have of the ground, or the sight which our eyes have of neighbouring objects, when we walk in a reverie. In these cases the sensations, though obscure, exist; for they

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[10] Prichard, *Vital Prin.* p. 126.

serve to balance and guide us as we walk. In other cases, our sensations are distinctly and directly the objects of our attention.

But our Sensations, as we have already said, we ascribe as Qualities to external objects. By our senses we perceive objects, and thus our *sensations* become *perceptions*. We have not only the sensation of *round, purple,* and *green,* repeated and varied, but the perception of a *bunch of grapes* partly ripe and partly unripe. We have not only sensations of noise and of variously-coloured specks rapidly changing their places, but we have perceptions, by sound and sight, of a stone rolling down the hill and crushing the shrubs in its path. We scarcely ever dwell upon our Sensations; our thoughts are employed upon Objects. We regard the impressions upon our nerves, not for what they *are,* but for what they *tell* us.

But in what Language do the impressions upon the nerves thus speak to us of an external world,—of the forms and qualities and actions of objects? How is it that by the aid of our nervous system we become acquainted not only with impressions but with *things;* that we learn not only the relation of objects to us, but to one another?

15. It has been shown at some length in the previous Books, that the mode in which Sensations are connected in our minds so as to convey to us the knowledge of Objects and their Relations, is by being contemplated with reference to *Ideas.* Our Sensations, connected by the Idea of Space, become Figures; connected by the Idea of Time, they become Causes and Effects; connected by the Idea of Resemblance, they become Individuals and Kinds; connected by the Idea of Organization, they become Living Things. It has been shown that without these Ideas there can be no connexion among our sensations, and therefore no perception of Figure, Action, Kind, or in short, of bodies under any aspect whatever. Sensations are the rude *Matter* of our perceptions; and are nothing, except so far as they have *Form* given them by Ideas.

But thus moulded by our Ideas, Sensation becomes the source of an endless store of important Knowledge of every possible kind.

16. But one of the most obvious uses of our perceptions and our knowledge is to direct our Actions. It is suitable to the condition of our being that when we perceive a bunch of grapes, we should be able to pluck and eat the ripe ones; that when we perceive a stone rushing down the side of a hill, we should be able to move so as to avoid it. And this must be done by moving our limbs; in short, by the use of our muscles. And thus Sensation leads, not directly, but through the medium of Ideas, to muscular Contraction. I say that sensation and Muscular action are in such cases connected through the medium of Ideas. For when we proceed to pluck the grape which we see, the *sensation* does not determine the motion of the hand by any necessary geometrical or mechanical conditions, as an impression made upon a machine determines its motions; but the *perception* leads us to stretch forth the hand to that part of space, wherever it is, where we *know* that the grape is; and this, not in any determinate path, but, it may be, avoiding or removing intervening obstacles, which we also *perceive.* There is in every such case a connexion between the sensation and the resulting action, not of a material but of a mental kind. The cause and the effect are bound together, not by physical but by intellectual ties.

17. And thus in such cases, between the two *vital* operations, Sensation and Muscular Action, there intervenes, as an intermediate step, Perception or Knowledge, which is not merely vital but *ideal.* But this is not all; there is still another mental part of the process which may be readily distinguished from that which we have described. An act of the *Will*, a Volition, is that, in the Mind, which immediately determines the action of the Muscles of the Body. And thus Will intervenes between Knowledge and Action; and the cycle of operations which take place when animals act with reference to external objects is

this:—Sensation, Perception, Volition, Muscular Contraction.

18. To attempt further to analyse the mental part of this cycle does not belong to the present part of our work. But we may remark here, as we have already remarked in the History[11], how irresistibly we are led by physiological researches into the domain of thought and mind. We pass from the body to the soul, from physics to metaphysics; from biology to psychology; from things to persons; from nouns to pronouns. I have there noticed the manner in which Cuvier expresses this transition by the introduction of the pronoun: 'The impression of external objects upon the ME, the production of a sensation, of an image, is a mystery impenetrable to our thoughts.'

19. But to return to the merely biological part of our speculations. We have arrived, it will be perceived, at this result: that in animal actions there intervenes between the two terms of Sensation and Muscular Contraction, an intermediate process; which may be described as a communication to and from a Center. The Center is the seat of the sentient and volent faculties, and is of a *hyperphysical* nature. But the existence of such a Center as a necessary element in the functions of the *animal* life is a truth which is important in biology. This indeed may be taken as the peculiar character of animal, as distinguished from merely *organic* powers. Accordingly, it is so stated by Bichat. For although he superfluously, as I have tried to show, introduces into his list of vital powers an organic sensibility, he still draws the distinction of which I have spoken; 'in the animal life, Sensibility is the faculty of receiving an Impression *plus* that of referring it to a common Center[12].'

20. But since Sensibility and Contractility are thus connected by reference to a common Center, we may ask, before quitting the subject, what are the different forms which this reference assumes? Is the connexion

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[11] *Hist. Ind. Sc.* b. xvii. c. v. s. 2.

[12] *Life and Death*, p. 84.

always attended by the distinct steps of Knowledge and Will,—by a clear act of consciousness, as in the case which we have taken, of plucking a grape; or may these steps become obscure, or vanish altogether?

We need not further illustrate the *conscious* connexion. Such actions as we have described are called *voluntary* actions. In extreme cases, the mental part of the process is obvious enough. But we may gradually pass from these to cases in which the mental operation is more and more obscure.

In walking, in speaking, in eating, in breathing, our muscular exertions are directed by our sensations and perceptions: yet in such processes, how dimly are we conscious of perceptive and directive power! How the mind should be able to exercise such a power, and yet should be scarcely or not at all conscious of its exercise, is a very curious problem. But in all or in most of the instances just mentioned, the solution of this problem appears to depend upon psychological rather than biological principles, and therefore does not belong to this place.

21. But in cases at the other extreme (unconscious actions) the mental part of the operation vanishes altogether. In many animals, even after decapitation, the limbs shrink when irritated. The motions of the iris are determined by the influence of light on our eyes, without our being aware of the motions. Here Sensations produce Motions, but with no trace of intervening Perception or Will. The Sensation appears to be *reflected* back from the central element of animal life, in the form of a Muscular Contraction; but in this case the Sensation is not modified or regulated by any *Idea*. These reflected motions have no reference to relations of space or force among surrounding objects. They are blind and involuntary, like the movements of convulsion, depending for direction and amount only on the position and circumstances of the limb itself with its muscles. Here the Centre from which the reflection takes place is merely *animal*, not intellectual.

In this case some physiologists have doubted whether the reflection of the sensation in the form of a muscular

contraction does really take place from the Center; and have conceived that sensorial impressions might affect motor nerves without any communication with the nervous Center. But on this subject we may, I conceive, with safety adopt the decision of Professor Müller, deliberately given after a careful examination of the subject: 'When impressions made by the action of external stimuli on sensitive nerves give rise to motions in other parts, these motions are never the result of the *direct* reaction of the sensitive and motor fibres of the nerves on each other; the irritation is conveyed by the sensitive fibres to the brain and spinal cord, and is by these communicated to the motor fibres.'

22. Thus we have two extreme cases of the connexion of sensation with muscular action; in one of which the connexion clearly *is*, and in the other it as clearly *is not*, determined by relations of Ideas, in its transit through the nervous Center. There is another highly curious case standing intermediate between these two, and extremely difficult to refer to either. I speak of the case of *Instinct.*

Instinct leads to actions which are *such as if they were determined by Ideas.* The lamb follows its mother by instinct; but the motions by which it does this, the special muscular exertions, depend entirely upon the geometrical and mechanical relations of external bodies, as the form of the ground, and the force of the wind. The contractions of the muscles which are requisite in order that the creature may obey its instinct, vary with every variation of these external conditions;—are not determined by any rule or necessity, but by properties of Space and Force. Thus the action is not governed by Sensations directly, but by sensations moulded by Ideas. And the same is the case with other cases of instinct. The dog hunts by instinct; but he hunts certain kinds of animals merely, thus showing that his instinct acts according to Resemblances and Differences; he crosses the field repeatedly to find the track of his prey by scent; thus recognizing the relations of Space with reference to the track; he leaps, adjusting his Force to

the distance and height of the leap with mechanical precision; and thus he practically recognizes the Ideas of Resemblance, Space, and Force.

But have animals such Ideas? In any proper sense in which we can speak of possessing Ideas, it appears plain that they have not. Animals cannot, at any time, be said properly to possess ideas, for ideas imply the possibility of *speculative* knowledge.

23. But even if we allow to animals only the *practical* possession of Ideas, we have still a great difficulty remaining. In the case of man, his ideas are unfolded gradually by his intercourse with the external world. The child learns to distinguish forms and positions by a repeated and incessant use of his hands and eyes; he learns to walk, to run, to leap, by slow and laborious degrees; he distinguishes one man from another, and one animal from another, only after repeated mistakes. Nor can we conceive this to be otherwise. How should the child know at once what muscles he is to exert in order to touch with his hand a certain visible object? How should he know what muscles to exert that he may stand and not fall, till he has tried often? How should he learn to direct his attention to the differences of different faces and persons, till he is roused by some memory, or hope which implies memory? It seems to us as if the sensations could not, without considerable practice, be rightly referred to Ideas of Space, Force, Resemblance, and the like.

Yet that which thus appears impossible, is in fact done by animals. The lamb almost immediately after its birth follows its mother, accommodating the actions of its muscles to the form of the ground. The chick, just escaped from the shell, picks up a minute insect, directing its beak with the greatest accuracy. Even the human infant seeks the breast and exerts its muscles in sucking, almost as soon as it is born. Hence, then, we see that Instinct produces at once actions regulated by Ideas, or, at least, which take place *as if* they were regulated by Ideas; although the Ideas cannot have been developed by exercise, and only appear to exist so far as such actions are concerned.

24. The term *Instinct* may properly be opposed to *Insight.* The former implies an inward principle of action, implanted within a creature and practically impelling it, but not capable of being developed into a subject of contemplation. While the instinctive actions of animals are directed by such a principle, the deliberate actions of man are governed by insight: he can contemplate the ideal relations on which the result of his action depends. He can in his mind map the path he will follow, and estimate the force he will exert, and class the objects he has to deal with, and determine his actions by the relations which he thus has present to his mind. He thus possesses Ideas not only practically, but speculatively. And knowing that the Ideas by which he commonly directs his actions, Space, Cause, Resemblance, and the like, have been developed to that degree of clearness in which he possesses them by the assiduous exercise of the senses and the mind from the earliest stage of infancy, and that these Ideas are capable of being still further unfolded into long trains of speculative truth, he is unable to conceive the manner in which animals possess such Ideas as their instinctive actions disclose:—Ideas which neither require to be unfolded nor admit of unfolding; which are adequate for practical purposes without any previous exercise, and inadequate for speculative purposes with whatever labour cultivated.

I have ventured to make these few remarks on Instinct since it may, perhaps, justly be considered as the last province of Biology, where we reach the boundary line of Psychology. I have now, before quitting this subject, only one other principle to speak of.

CHAPTER VI.

OF THE IDEA OF FINAL CAUSES.

1. BY an examination of those notions which enter into all our reasonings and judgments on living things, it appeared that we conceive animal life as a vortex or cycle of moving matter in which the form of the vortex determines the motions, and these motions again support the form of the vortex: the stationary parts circulate the fluids, and the fluids nourish the permanent parts. Each portion ministers to the others, each depends upon the other. The parts make up the whole, but the existence of the whole is essential to the preservation of the parts. But parts existing under such conditions are *organs*, and the whole is *organized*. This is the fundamental conception of organization. 'Organized beings,' says the physiologist[1], 'are composed of a number of essential and mutually dependent parts.' 'An organized product of nature,' says the great metaphysician[2], 'is that in which all the parts are mutually ends and means.'

2. It will be observed that we do not content ourselves with saying that in such a whole, all the parts are *mutually dependent*. This might be true even of a mechanical structure; it would be easy to imagine a framework in which each part should be necessary to the support of each of the others; for example, an arch of several stones. But in such a structure, the parts have no properties which they derive from the whole. They are beams or stones when separate; they are no more when joined. But the same is not the case in an organized whole. The limb of an animal separated

[1] Müller, *Elem.* p. 18,

[2] Kant, *Urtheilskraft*, p. 296.

from the body, loses the properties of a limb, and soon ceases to retain even its form.

3. Nor do we content ourselves with saying that the parts are *mutually causes and effects.* This is the case in machinery. In a clock, the pendulum by means of the escapement causes the descent of the weight, the weight by the same escapement keeps up the motion of the pendulum. But things of this kind may happen by accident. Stones slide from a rock down the side of a hill and cause it to be smooth; the smoothness of the slope causes stones still to slide. Yet no one would call such a slide an organized system. The system is organized, when the effects which take place among the parts are *essential to our conception of the whole;* when the whole would not *be* a whole, nor the parts, parts, except these effects were produced; when the effects not only happen in fact, but are included in the idea of the object; when they are not only seen, but foreseen; not only expected, but intended: in short when, instead of being causes and effects, they are *ends* and *means,* as they are termed in the above definition.

Thus we necessarily include, in our Idea of Organization, the notion of an End, a Purpose, a Design; or, to use another phrase which has been peculiarly appropriated in this case, a *Final Cause.* This idea of a Final Cause is an essential condition in order to the pursuing our researches respecting organized bodies.

4. This Idea of Final Cause is not *deduced* from the phenomena by reasoning, but is *assumed* as the only condition under which we can reason on such subjects at all. We do not deduce the Idea of Space, or Time, or efficient Cause from the phenomena about us, but necessarily look at phenomena as subordinate to these Ideas from the beginning of our reasoning. It is true, our ideas of relations of Space, and Time, and Force, may become much more clear by our familiarizing ourselves with particular phenomena: but still, the Fundamental Ideas are not generated, but unfolded; not extracted from the external world, but evolved from the world within. In like manner, in the contemplation of organic structures, we consider

each part as subservient to some use, and we cannot study the structure as organic without such a conception. This notion of adaptation,—this Idea of an End,—may become much more clear and impressive by seeing it exemplified in particular cases. But still, though suggested and evoked by special cases, it is not furnished by them. If it be not supplied by the mind itself, it can never be logically deduced from the phenomena. It is not a portion of the facts which we study, but it is a principle which connects, includes, and renders them intelligible; as our other Fundamental Ideas do the classes of facts to which they respectively apply.

5. This has already been confirmed by reference to fact; in the History of Physiology, I have shown that those who studied the structure of animals were irresistibly led to the conviction that the parts of this structure have each its end or purpose;—that each member and organ not merely produces a certain effect or answers a certain use, but is so framed as to impress us with the persuasion that it was constructed *for* that use:—that it was *intended* to produce the effect. It was there seen that this persuasion was repeatedly expressed in the most emphatic manner by Galen;—that it directed the researches and led to the discoveries of Harvey;—that it has always been dwelt upon as a favourite contemplation, and followed as a certain guide, by the best anatomists;—and that it is inculcated by the physiologists of the profoundest views and most extensive knowledge of our own time. All these persons have deemed it a most certain and important principle of physiology, that in every organized structure, plant or animal, each intelligible part has its allotted office:—each organ is designed for its appropriate function:—that nature, in these cases, produces nothing in vain: that, in short, each portion of the whole arrangement has its *final cause;* an End to which it is adapted, and in this End, the reason that it is where and what it is.

6. This Notion of Design in organized bodies must, I say, be supplied by the student of organization out of his own mind: a truth which will become clearer if

we attend to the most conspicuous and acknowledged instances of *design*. The structure of the Eye, in which the parts are curiously adjusted so as to produce a distinct image on the retina, as in an optical instrument; —the Trochlear Muscle of the eye, in which the tendon passes round a support and turns back, like a rope round a pulley;—the prospective contrivances for the preservation of animals, provided long before they are wanted, as the Milk of the mother, the Teeth of the child, the Eyes and Lungs of the fœtus:—these arrangements, and innumerable others, call up in us a persuasion that Design has entered into the plan of animal form and progress. And if we bring in our minds this conception of Design, nothing can more fully square with and fit it, than such instances as these. But if we did not already possess the Idea of Design; —if we had not had our notion of mechanical contrivance awakened by inspection of optical instruments, or pulleys, or in some other way;—if we had never been conscious ourselves of providing for the future;—if this were the case, we could not recognize contrivance and prospectiveness in such instances as we have referred to. The facts are, indeed, admirably in accordance with these conceptions, when the two are brought together: but the facts and the conceptions come together from different quarters—from without and from within.

7. We may further illustrate this point by referring to the relations of travellers who tell us that when consummate examples of human mechanical contrivance have been set before savages, they have appeared incapable of apprehending them as proofs of design. This shows that in such cases the Idea of Design had not been developed in the minds of the people who were thus unintelligent: but it no more proves that such an idea does not naturally and necessarily arise, in the progress of men's minds, than the confused manner in which the same savages apprehend the relations of space, or number, or cause, proves that these ideas do not naturally belong to their intellects. All men have these ideas; and it is because they can-

not help referring their sensations to such ideas, that they apprehend the world as existing in time and space, and as a series of causes and effects. It would be very erroneous to say that the belief of such truths is obtained by logical reasoning from facts. And in like manner we cannot logically deduce design from the contemplation of organic structures; although it is impossible for us, when the facts are clearly before us, not to find a reference to design operating in our minds.

8. Again; the evidence of the doctrine of Final Causes as a fundamental principle of Biology may be obscured and weakened in some minds by the constant habit of viewing this doctrine with suspicion as unphilosophical and at variance with Morphology. By cherishing such views, it is probable that many persons, physiologists and others, have gradually brought themselves to suppose that many or most of the arrangements which are familiarly adduced as instances of design may be accounted for, or explained away;—that there is a certain degree of prejudice and narrowness of comprehension in that lively admiration of the adaptation of means to ends which common minds derive from the spectacle of organic arrangements. And yet, even in persons accustomed to these views, the strong and natural influence of the Idea of a Final Cause, the spontaneous recognition of the relation of Means to an End as the assumption which makes organic arrangements intelligible, breaks forth when we bring before them a new case, with regard to which their genuine convictions have not yet been modified by their intellectual habits. I will offer, as an example which may serve to illustrate this, the discoveries recently made with regard to the process of Suckling in the Kangaroo. In the case of this, as of other pouched animals, the young animal is removed, while very small and imperfectly formed, from the womb to the pouch, in which the teats are, and is there placed with its lips against one of the nipples. But the young animal taken altogether is not so large as the nipple, and is therefore incapable of sucking after the manner of common mammals. Here is a difficulty:

how is it overcome?—By an appropriate *contrivance:* the nipple, which in common mammals is not furnished with any muscle, is in the kangaroo provided with a powerful extrusory muscle by which the mother can inject the milk into the mouth of her offspring. And again; in order to give attachment to this muscle there is a bone which is not found in animals of other kinds. But this mode of solving the problem of suckling so small a creature introduces another difficulty. If the milk is injected into the mouth of the young one, without any action of its own muscles, what is to prevent the fluid entering the windpipe and producing suffocation? How is this danger avoided?—By another appropriate *contrivance:* there is a funnel in the back of the throat by which the air passage is completely separated from the passage for nutriment, and the injected milk passes in a divided stream on each side of the larynx to the œsophagus[3]. And as if to show that this apparatus is really formed with a view to the wants of the young one, the structure alters in the course of the animal's growth; and the funnel, no longer needed, is modified and disappears.

With regard to this and similar examples, the remark which I would urge is this:—that no one, however prejudiced or unphilosophical he may in general deem the reference to Final Causes, can, at the first impression, help regarding this curious system of arrangement as the Means to an End. So contemplated, it becomes significant, intelligible, admirable: without such a principle, it is an unmeaning complexity, a collection of contradictions, producing an almost impossible result by a portentous conflict of chances. The parts of this apparatus cannot have produced one another: one part is in the mother; another part in the young one: without their harmony they could not be effective; but nothing except design can operate to make them harmonious. They are *intended* to work together; and we cannot resist the conviction of this intention when the facts first come before us. Perhaps

[3] Mr. Owen, in *Phil. Trans.* 1834, p. 348.

there may hereafter be physiologists who, tracing the gradual development of the parts of which we have spoken, and the analogies which connect them with the structures of other animals, may think that this development, these analogies, account for the conformation we have described; and may hence think lightly of the explanation derived from the reference to Final Causes. Yet surely it is clear, on a calm consideration of the subject, that the latter explanation is not disturbed by the former; and that the observer's first impression, that this is 'an irrefragable evidence of creative foresight[4],' can never be obliterated; however much it may be obscured in the minds of those who confuse this view by mixing it with others which are utterly heterogeneous to it, and therefore cannot be contradictory.

9. I have elsewhere[5] remarked how physiologists, who thus look with suspicion and dislike upon the introduction of Final Causes into physiology, have still been unable to exclude from their speculations causes of this kind. Thus Cabanis says[6], 'I regard with the great Bacon, the philosophy of Final Causes as sterile; but I have elsewhere acknowledged that it was very difficult for the most cautious man never to have recourse to them in his explanations.' Accordingly, he says, 'The partisans of Final Causes nowhere find arguments so strong in favour of their way of looking at nature as in the laws which preside and the circumstances of all kinds which concur in the reproduction of living races. In no case do the means employed appear so clearly relative to the end.' And it would be easy to find similar acknowledgments, express or virtual, in other writers of the same kind. Thus Bichat, after noting the difference between the organic sensibility by which the organs are made to perform their offices, and the animal sensibility of which the

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[4] Mr. Owen, in *Phil. Trans.* 1834, p. 349.

[5] *Bridgewater Treatise*, p. 352.

[6] *Rapports du Physique et du Moral*, i. 299.

nervous center is the seat, says[7], 'No doubt it will be asked, *why*'—that is, as we shall see, for what *end*—'the organs of internal life have received from nature an inferior degree of sensibility only, and why they do not transmit to the brain the impressions which they receive, while all the acts of the animal life imply this transmission? The reason is simply this, that all the phenomena which establish our connexions with surrounding objects *ought to be*, and are in fact, under the influence of the Will; while all those which serve for the purpose of assimilation only, escape, and *ought* indeed to escape, such influence.' The *reason* here assigned is the Final Cause; which, as Bichat justly says, we cannot help asking for.

10. Again; I may quote from the writer last mentioned another remark, which shows that in the organical sciences, and in them alone, the Idea of forces as Means acting to an End, is inevitably assumed and acknowledged as of supreme authority. In Biology alone, observes Bichat[8], have we to contemplate the state of *Disease*. 'Physiology is to the movements of living bodies, what astronomy, dynamics, hydraulics, &c., are to those of inert matter: but these latter sciences have no branches which correspond to them as Pathology corresponds to Physiology. For the same reason all notion of a Medicament is repugnant to the physical sciences. A Medicament has for its object to bring the properties of the system back to their Natural Type; but the physical properties never depart from this Type, and have no need to be brought back to it: and thus there is nothing in the physical sciences which holds the place of Therapeutick in Physiology.' Or, as we might express it otherwise, of inert forces we have no conception of what they *ought to do*, except what they *do*. The forces of gravity, elasticity, affinity, never act in a *diseased* manner; we never conceive them as failing in their purpose; for we do not conceive them as having any purpose which is answered by one mode of their action rather than

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[7] *Life and Death*, (trans.) p. 32.

[8] *Anatomie Générale*, i. liii.

another. But with *organical* forces the case is different; they are necessarily conceived as acting for the preservation and development of the system in which they reside. If they do not do this, they fail, they are deranged, diseased. They have for their object to conform the living being to a certain type; and if they cause or allow it to deviate from this type, their action is distorted, morbid, contrary to the ends of nature. And thus this conception of organized beings as susceptible of disease, implies the recognition of a state of health, and of the organs and the vital forces as means for preserving this normal condition. The state of health, and of perpetual development, is necessarily contemplated as the Final Cause of the processes and powers with which the different parts of plants and animals are endowed.

11. This Idea of a Final Cause is applicable as a fundamental and regulative idea to our speculations concerning organized creatures only. That there is a purpose in many other parts of the creation, we find abundant reason to believe, from the arrangements and laws which prevail around us. But this persuasion is not to be allowed to regulate and direct our reasonings with regard to inorganic matter, of which conception the relation of means and end forms no essential part. In mere Physics, Final Causes, as Bacon has observed, are not to be admitted as a principle of reasoning. But in the organical sciences, the assumption of design and purpose in every part of every whole, that is, the pervading idea of Final Cause, is the basis of sound reasoning and the source of true doctrine.

12. The Idea of Final Cause, of end, purpose, design, intention, is altogether different from the Idea of Cause, as Efficient Cause, which we formerly had to consider; and on this account the use of the word *Cause* in this phrase has been objected to. If the idea be clearly entertained and steadily applied, the word is a question of subordinate importance. The term *Final Cause* has been long familiarly used, and appears not likely to lead to confusion.

13. The consideration of Final Causes, both in physiology and in other subjects, has at all times attracted much attention, in consequence of its bearing upon the belief of an Intelligent Author of the Universe. I do not intend, in this place, to pursue the subject far in this view: but there is one antithesis of opinion, already noticed in the History of Physiology, on which I will again make a few remarks[9].

It has appeared to some persons that the mere aspect of order and symmetry in the works of nature—the contemplation of comprehensive and consistent law—is sufficient to lead us to the conception of a design and intelligence producing the order and carrying into effect the law. Without here attempting to decide whether this is true, we may discern, after what has been said, that the conception of Design, arrived at in this manner, is altogether different from that Idea of Design which is suggested to us by organized bodies, and which we describe as the doctrine of Final Causes. The regular form of a crystal, whatever beautiful symmetry it may exhibit, whatever general laws it may exemplify, does not prove design in the same manner in which design is proved by the provisions for the preservation and growth of the seeds of plants, and of the young of animals. The law of universal gravitation, however wide and simple, does not impress us with the belief of a purpose, as does that propensity by which the two sexes of each animal are brought together. If it could be shown that the symmetrical structure of a flower results from laws of the same kind as those which determine the regular forms of crystals, or the motions of the planets, the discovery might be very striking and important, but it would not at all come under our idea of Final Cause.

14. Accordingly, there have been, in modern times, two different schools of physiologists, the one proceeding upon the idea of Final Causes, the other school

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[9] *Hist. Ind. Sc.* b. xvii. c. viii. On the Doctrine of Final Causes in Physiology.

seeking in the realm of organized bodies wide laws and analogies from which that idea is excluded. All the great biologists of preceding times, and some of the greatest of modern times, have belonged to the former school; and especially Cuvier, who may be considered as the head of it. It was solely by the assiduous application of this principle of Final Cause, as he himself constantly declared, that he was enabled to make the discoveries which have rendered his name so illustrious, and which contain a far larger portion of important anatomical and biological truth than it ever before fell to the lot of one man to contribute to the science.

The opinions which have been put in opposition to the principle of Final Causes have, for the most part, been stated vaguely and ambiguously. Among the most definite of such principles, is that which, in the History of the subject, I have termed the Principle of Metamorphosed and Developed Symmetry, upon which has been founded the science of Morphology.

The reality and importance of this principle are not to be denied by us: we have shown how they are proved by its application in various sciences, and especially in botany. But those advocates of this principle who have placed it in antithesis to the doctrine of Final Causes, have, by this means, done far more injustice to their own favourite doctrine than damage to the one which they opposed. The adaptation of the bones of the skeleton to the muscles, the provision of fulcrums, projecting processes, channels, so that the motions and forces shall be such as the needs of life require, cannot possibly become less striking and convincing, from any discovery of general analogies of one animal frame with another, or of laws connecting the development of different parts. Whenever such laws are discovered, we can only consider them as the means of producing that adaptation which we so much admire. Our conviction that the Artist works intelligently, is not destroyed, though it may be modified and transferred, when we obtain a sight of his tools. Our discovery of laws cannot contradict our persuasion of ends; our Morphology cannot prejudice our Teleology.

15. The irresistible and constant apprehension of a purpose in the forms and functions of animals has introduced into the writings of speculators on these subjects various forms of expression, more or less precise, more or less figurative; as, that 'animals are framed with a view to the part which they have to play;'—that 'nature does nothing in vain;' that 'she employs the best means for her ends;' and the like. However metaphorical or inexact any of these phrases may be in particular, yet taken altogether, they convey, clearly and definitely enough to preclude any serious errour, a principle of the most profound reality and of the highest importance in the organical sciences. But some adherents of the morphological school of which I have spoken reject, and even ridicule, all such modes of expression. 'I know nothing,' says M. Geoffroy Saint Hilaire, 'of animals which have to play a part in nature. I cannot make of nature an intelligent being who does nothing in vain; who acts by the shortest mode; who does all for the best.' The philosophers of this school, therefore, do not, it would seem, feel any of the admiration which is irresistibly excited in all the rest of mankind at the contemplation of the various and wonderful adaptations for the preservation, the enjoyment, the continuation of the creatures which people the globe;—at the survey of the mechanical contrivances, the chemical agencies, the prospective arrangements, the compensations, the minute adaptations, the comprehensive interdependencies, which zoology and physiology have brought into view, more and more, the further their researches have been carried. Yet the clear and deep-seated conviction of the reality of these provisions, which the study of anatomy produces in its most profound and accurate cultivators, cannot be shaken by any objections to the metaphors or terms in which this conviction is clothed. In regard to the Idea of a Purpose in organization, as in regard to any other idea, we cannot fully express our meaning by phrases borrowed from any extraneous source; but that impossibility arises precisely from the circumstance of its being a Fundamental Idea which is inevitably assumed in our

representation of each special fact. The same objection has been made to the idea of mechanical *force*, on account of its being often expressed in metaphorical language; for writers have spoken of an *energy*, *effort*, or *solicitation* to motion; and bodies have been said to be *animated* by a force. Such language, it has been urged, implies volition, and the act of animated beings. But the idea of Force as distinct from mere motion,—as the Cause of motion, or of tendency to motion,—is not on that account less real. We endeavour in vain to conduct our mechanical reasonings without the aid of this idea, and must express it as we can. Just as little can we reason concerning organized beings without assuming that each part has its function, each function its purpose; and so far as our phrases imply this, they will not mislead us, however inexact, or however figurative they be.

16. The doctrine of a purpose in Organization has been sometimes called the doctrine of *the Conditions of Existence;* and has been stated as teaching that each animal must be so framed as to contain in its structure the Conditions which its existence requires. When expressed in this manner, it has given rise to the objection, that it merely offers an identical proposition; since no animal *can* exist without such conditions. But in reality, such expressions as those just quoted give an inadequate statement of the Principle of a Final Cause. For we discover in innumerable cases, arrangements in an animal, of which we see, indeed, that they are subservient to its well being; but the nature of which we never should have been able at all to conjecture, from considering what was necessary to its existence, and which strike us, no less by their unexpectedness than by their adaptation: so far are they from being presented by any perceptible necessity. Who would venture to say that the trochlear muscle, or the power of articulate speech, must occur in man, because they are the necessary conditions of his existence? When, indeed, the general scheme and mode of being of an animal are known, the expert and profound anatomist can reason concerning the proportions and

form of its various parts and organs, and prove in some measure what their relations must be. We can assert, with Cuvier, that certain forms of the viscera require certain forms of the teeth, certain forms of the limbs, certain powers of the senses. But in all this, the functions of self-nutrition and digestion are supposed already existing as ends: and it being taken for granted, as the only conceivable basis of reasoning, that the organs are means to these ends, we may discover what modifications of these organs are necessarily related to and connected with each other. Instead of terming this rule of speculation merely 'the Principle of the Conditions of Existence,' we might term it 'the Principle of the conditions of organs as *Means* adapted to animal existence as their *End*.' And how far this principle is from being a mere barren truism, the extraordinary discoveries made by the great assertor of the principle, and universally assented to by naturalists, abundantly prove. The vast extinct creation which is recalled to life in Cuvier's great work, the *Ossemens Fossiles*, cannot be the consequence of a mere identical proposition.

17. It has been objected, also, that the doctrine of Final Causes supposes us to be acquainted with the intentions of the Creator; which, it is insinuated, is a most presumptuous and irrational basis for our reasonings. But there can be nothing presumptuous or irrational in reasoning on that basis, which if we reject, we cannot reason at all. If men really can discern, and cannot help discerning, a design in certain portions of the works of creation, this perception is the soundest and most satisfactory ground for the convictions to which it leads. The Ideas which we necessarily employ in the contemplation of the world around us, afford us the only natural means of forming any conception of the Creator and Governor of the Universe; and if we are by such means enabled to elevate our thoughts, however inadequately, towards Him, where is the presumption of doing so? or rather, where is the wisdom of refusing to open our minds to contemplations so animating and elevating, and yet

so entirely convincing? We possess the ideas of Time and Space, under which all the objects of the universe present themselves to us; and in virtue of these ideas thus possessed, we believe the Creator to be eternal and omnipotent. When we find that we, in like manner, possess the idea of a Design in Creation, and that with regard to ourselves, and creatures more or less resembling ourselves, we cannot but contemplate their constitution under this idea, we cannot abstain from ascribing to the Creator the infinite profundity and extent of design to which all these special instances belong as parts of a whole.

18. I have here considered Design as manifest in organization only: for in that field of speculation it is forced upon us as contained in all the phenomĕna, and as the only mode of our understanding them. The existence of Final Causes has often been pointed out in other portions of the creation;—for instance, in the apparent adaptations of the various parts of the earth and of the solar system to each other and to organized beings. In these provinces of speculation, however, the principle of Final Causes is no longer the basis and guide, but the sequel and result of our physical reasonings. If in looking at the universe, we follow the widest analogies of which we obtain a view, we see, however dimly, reason to believe that all its laws are adapted to each other, and intended to work together for the benefit of its organic population, and for the general welfare of its rational tenants. On this subject, however, not immediately included in the principle of Final Causes as here stated, I shall not dwell. I will only make this remark; that the assertion appears to be quite unfounded, that as science advances from point to point, Final Causes recede before it, and disappear one after the other. The principle of design changes its mode of application indeed, but it loses none of its force. We no longer consider particular facts as produced by special interpositions, but we consider design as exhibited in the establishment and adjustment of the laws by which particular facts are produced. We do not look upon each particular

cloud as brought near us that it may drop fatness on our fields; but the general adaptation of the laws of heat, and air, and moisture, to the promotion of vegetation, does not become doubtful. We do not consider the sun as less intended to warm and vivify the tribes of plants and animals, because we find that, instead of revolving round the earth as an attendant, the earth along with other planets revolves round him. We are rather, by the discovery of the general laws of nature, led into a scene of wider design, of deeper contrivance, of more comprehensive adjustments. Final causes, if they appear driven further from us by such an extension of our views, embrace us only with a vaster and more majestic circuit: instead of a few threads connecting some detached objects, they become a stupendous net-work, which is wound round and round the universal frame of things.

19. I now quit the subject of Biology, and with it the circle of sciences depending upon separate original Ideas and permanent relations. If from the general relations which permanently prevail and constantly recur among the objects around us, we turn to the inquiry of what has actually happened,—if from Science we turn to History,—we find ourselves in a new field. In this region of speculation we can rarely obtain a complete and scientific view of the connexion between objects and events. The past History of Man, of the Arts, of Languages, of the Earth, of the Solar System, offers a vast series of problems, of which perhaps not one has been rigorously solved. Still, man, as his speculative powers unfold themselves, cannot but feel prompted and invited to employ his thoughts even on these problems. He cannot but wish and endeavour to understand the connexion between the successive links of such chains of events. He attempts to form a Science which shall be applicable to each of these Histories; and thus he begins to construct the class of sciences to which I now, in the last place, proceed.

BOOK X.

THE

PHILOSOPHY

OF

PALÆTIOLOGY.

Τὴν μὲν οὖν τοιαύτην Αἰτιολογίαν ἧττον ἄν τις ἀποδέξαιτο· μᾶλλον δ' ἀπὸ τῶν φανερωτέρων καὶ τῶν καθ' ἡμέραν τρόπον τινὰ ὁρωμένων ἀναπτέον τὸν λόγον. Καὶ γὰρ κατακλυσμοί, καὶ σεισμοί, καὶ ἀναφυσήματα, καὶ ἀποιδήσεις τῆς ὑφάλου γῆς, μετεωρίζουσι καὶ τὴν θάλατταν· αἱ δὲ συνιζήσεις ταπεινοῦσιν αὐτήν.

STRABO, *Geogr.* I. p. 54.

It is therefore, not so much what these forms of the earth actually are, as what they are continually becoming, that we have to observe; nor is it possible thus to observe them without an instinctive reference to the first state out of which they have been brought....Yet to such questions continually suggesting themselves, it is never possible to give a complete answer. For a certain distance, the past work of existing forces can be traced; but then gradually the mist gathers, and the footsteps of more gigantic agencies are traceable in the darkness; and still as we endeavour to penetrate further and further into departed time, the thunder of the Almighty power sounds louder and louder, and the clouds gather broader and more fearfully, until at last the Sinai of the world is seen altogether upon a smoke, and the fence of its foot is reached, where none can break through.

RUSKIN, *Modern Painters*, Vol. IV. p. 143.

BOOK X.

THE PHILOSOPHY OF PALÆTIOLOGY.

CHAPTER I.

OF PALÆTIOLOGICAL SCIENCES IN GENERAL.

1. I HAVE already stated in the *History of the Sciences*[1], that the class of Sciences which I designate as *Palætiological* are those in which the object is to ascend from the present state of things to a more ancient condition, from which the present is derived by intelligible causes. As conspicuous examples of this class we may take Geology, Glossology or Comparative Philology, and Comparative Archæology. These provinces of knowledge might perhaps be intelligibly described as *Histories;* the History of the Earth,—the History of Languages,—the History of Arts. But these phrases would not fully describe the sciences we have in view; for the object to which we now suppose their investigations to be directed is, not merely to ascertain what the series of events has been, as in the common forms of History, but also how it has been brought about. These sciences are to treat of causes as well as of effects. Such researches might be termed *Philosophical History;* or, in order to mark more distinctly that the *causes* of events are the leading object of attention, *Ætiological History.* But since

[1] B. xviii. Introd.

it will be more convenient to describe this class of sciences by a single appellation, I have taken the liberty of proposing to call them[2] the *Palætiological* Sciences.

While *Palæontology* describes the beings which have lived in former ages without investigating their causes, and *Ætiology* treats of causes without distinguishing historical from mechanical causation; *Palætiology* is a combination of the two sciences; exploring, by means of the second, the phenomena presented by the first. The portions of knowledge which I include in this term are palæontological ætiological sciences.

2. All these sciences are connected by this bond;—that they all endeavour to ascend to a past state, by considering what is the present state of things, and what are the causes of change. Geology examines the existing appearance of the materials which form the earth, infers from them previous conditions, and speculates concerning the forces by which one condition has been made to succeed another. Another science, cultivated with great zeal and success in modern times, compares the languages of different countries and nations, and by an examination of their materials and structure, endeavours to determine their descent from one another: this science has been termed *Comparative Philology*, or *Ethnography;* and by the French, *Linguistique*, a word which we might imitate in order to have a single name for the science, but the Greek derivative *Glossology* appears to be more convenient in its form. The progress of the Arts (Architecture and the like);—how one stage of the culture produced another; and how far we can trace their maturest and most complete condition to their earliest form in various nations;—are problems of great interest belonging to another subject, which we may for the present term *Compara-*

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[2] A philological writer, in a very interesting work (Mr. Donaldson, in his *New Cratylus*, p. 12), expresses his dislike of this word, and suggests that I must mean *palæ-ætiological*. I think the word is more likely to obtain currency in the more compact and euphonious form in which I have used it. It has been adopted by Mr. Winning, in his *Manual of Comparative Philology*, and more recently, by other writers.

tive Archæology. I have already noticed, in the *History*[3], how the researches into the origin of natural objects, and those relating to works of art, pass by slight gradations into each other; how the examination of the changes which have affected an ancient temple or fortress, harbour or river, may concern alike the geologist and the antiquary. Cuvier's assertion that the geologist is an antiquary of a new order, is perfectly correct, for both are palætiologists.

3. We are very far from having exhausted, by this enumeration, the class of sciences which are thus connected. We may easily point out many other subjects of speculation of the same kind. As we may look back towards the first condition of our planet, we may in like manner turn our thoughts towards the first condition of the solar system, and try whether we can discern any traces of an order of things antecedent to that which is now established; and if we find, as some great mathematicians have conceived, indications of an earlier state in which the planets were not yet gathered into their present forms, we have, in the pursuit of this train of research, a palætiological portion of Astronomy. Again, as we may inquire how languages, and how man, have been diffused over the earth's surface from place to place, we may make the like inquiry with regard to the races of plants and animals, founding our inferences upon the existing geographical distribution of the animal and vegetable kingdoms: and thus the Geography of Plants and of Animals also becomes a portion of Palætiology. Again, as we can in some measure trace the progress of Arts from nation to nation and from age to age, we can also pursue a similar investigation with respect to the progress of Mythology, of Poetry, of Government, of Law. Thus the philosophical history of the human race, viewed with reference to these subjects, if it can give rise to knowledge so exact as to be properly called *Science*, will supply Sciences belonging to the class I am now to consider.

[3] B. xviii. Introd.

4. It is not an arbitrary and useless proceeding to construct such a Class of Sciences. For wide and various as their subjects are, it will be found that they have all certain principles, maxims, and rules of procedure in common; and thus may reflect light upon each other by being treated of together. Indeed it will, I trust, appear, that we may by such a juxtaposition of different speculations, obtain most salutary lessons. And questions, which, when viewed as they first present themselves under the aspect of a special science, disturb and alarm men's minds, may perhaps be contemplated more calmly, as well as more clearly, when they are considered as general problems of palætiology.

5. It will at once occur to the reader that, if we include in the circuit of our classification such subjects as have been mentioned,—politics and law, mythology and poetry,—we are travelling very far beyond the material sciences within whose limits we at the outset proposed to confine our discussion of principles. But we shall remain faithful to our original plan; and for that purpose shall confine ourselves, in this work, to those palætiological sciences which deal with material things. It is true, that the general principles and maxims which regulate these sciences apply also to investigations of a parallel kind respecting the products which result from man's imaginative and social endowments. But although there may be a similarity in the general form of such portions of knowledge, their materials are so different from those with which we have been hitherto dealing, that we cannot hope to take them into our present account with any profit. Language, Government, Law, Poetry, Art, embrace a number of peculiar Fundamental Ideas, hitherto not touched upon in the disquisitions in which we have been engaged; and most of them involved in far greater perplexity and ambiguity, the subject of controversies far more vehement, than the Ideas we have hitherto been examining. We must therefore avoid resting any part of our philosophy upon sciences, or supposed sciences, which treat of such subjects. To attend to this caution,

is the only way in which we can secure the advantage we proposed to ourselves at the outset, of taking, as the basis of our speculations, none but systems of undisputed truths, clearly understood and expressed[4]. We have already said that we must, knowingly and voluntarily, resign that livelier and warmer interest which doctrines on subjects of Polity or Art possess, and content ourselves with the cold truths of the material sciences, in order that we may avoid having the very foundations of our philosophy involved in controversy, doubt, and obscurity.

6. We may remark, however, that the necessity of rejecting from our survey a large portion of the researches which the general notion of Palætiology includes, suggests one consideration which adds to the interest of our task. We began our inquiry with the trust that any sound views which we should be able to obtain respecting the nature of Truth in the physical sciences, and the mode of discovering it, must also tend to throw light upon the nature and prospects of knowledge of all other kinds;—must be useful to us in moral, political, and philological researches. We stated this as a confident anticipation; and the evidence of the justice of our belief already begins to appear. We have seen, in the last Book, that biology leads us to psychology, if we choose to follow the path; and thus the passage from the material to the immaterial has already unfolded itself at one point; and we now perceive that there are several large provinces of speculation which concern subjects belonging to man's immaterial nature, and which are governed by the same laws as sciences altogether physical. It is not our business here to dwell on the prospects which our philosophy thus opens to our contemplation; but we may allow ourselves, in this last stage of our pilgrimage among the foundations of the physical sciences, to be cheered and animated by the ray

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[4] See Introd. p. 9.

that thus beams upon us, however dimly, from a higher and brighter region.

But in our reasonings and examples we shall mainly confine ourselves to the physical sciences; and for the most part to Geology, which in the *History* I have put forwards as the best representative of the Palætiological Sciences.

CHAPTER II.

Of the Three Members of a Palætiological Science.

1. *Divisions of such Sciences.*—In each of the Sciences of this class we consider some particular order of phenomena now existing:—from our knowledge of the causes of change among such phenomena, we endeavour to infer the causes which have made this order of things what it is:—we ascend in this manner to some previous stage of such phenomena;—and from that, by a similar course of inference, to a still earlier stage, and to *its* causes. Hence it will be seen that each such science will consist of two parts,—the knowledge of the Phenomena, and the knowledge of their Causes. And such a division is, in fact, generally recognized in such sciences: thus we have History, and the Philosophy of History; we have Comparison of Languages, and the Theories of the Origin and Progress of Language; we have Descriptive Geology, and Theoretical or Physical Geology. In all these cases, the relation between the two parts in these several provinces of knowledge is nearly the same; and it may, on some occasions at least, be useful to express the distinction in a uniform or general manner. The investigation of Causes has been termed *Ætiology* by philosophical writers, and this term we may use, in contradistinction to the mere *Phenomenology* of each such department of knowledge. And thus we should have *Phenomenal Geology* and *Ætiological Geology*, for the two divisions of the science which we have above termed *Descriptive* and *Theoretical Geology*.

2. *The Study of Causes.*—But our knowledge respecting the causes which actually *have* produced any

order of phenomena must be arrived at by ascertaining what the causes of change in such matters *can* do. In order to learn, for example, what share earthquakes, and volcanoes, and the beating of the ocean against its shores, ought to have in our Theory of Geology, we must make out what effects these agents of change are able to produce. And this must be done, not hastily, or unsystematically, but in a careful and connected manner; in short, this study of the causes of change in each order of phenomena must become a distinct body of Science, which must include a large amount of knowledge, both comprehensive and precise, before it can be applied to the construction of a theory. We must have an Ætiology corresponding to each order of phenomena.

3. *Ætiology.*—In the History of Geology, I have spoken of the necessity for such an Ætiology with regard to geological phenomena: this necessity I have compared with that which, at the time of Kepler, required the formation of a separate science of Dynamics (the doctrine of the Causes of Motion), before Physical Astronomy could grow out of Phenomenal Astronomy. In pursuance of this analogy, I have there given the name of *Geological Dynamics* to the science which treats of the causes of geological change in general. But, as I have there intimated, in a large portion of the subject the changes are so utterly different in their nature from any modification of motion, that the term *Dynamics*, so applied, sounds harsh and strange. For in this science we have to treat, not only of the subterraneous forces by which parts of the earth's crust are shaken, elevated, or ruptured, but also of the causes which may change the climate of a portion of the earth's surface, making a country hotter or colder than in former ages; again, we have to treat of the causes which modify the forms and habits of animals and vegetables, and of the extent to which the effects of such causes can proceed; whether, for instance, they can extinguish old species and produce new. These and other similar investigations would not be naturally included in the notion of *Dynamics;* and therefore it

might perhaps be better to use the term *Ætiology* when we wish to group together all those researches which have it for their object to determine the laws of such changes. In the same manner the Comparison and History of Languages, if it is to lead to any stable and exact knowledge, must have appended to it an Ætiology, which aims at determining the nature and the amount of the causes which really do produce changes in language; as colonization, conquest, the mixture of races, civilization, literature, and the like. And the same rule applies to all sciences of this class. We shall now make a few remarks on the characteristics of such branches of science as those to which we are led by the above considerations.

4. *Phenomenology requires Classification. Phenomenal Geology.*—The Phenomenal portions of each science imply Classification, for no description of a large and varied mass of phenomena can be useful or intelligible without classification. A representation of phenomena, in order to answer the purposes of science, must be systematic. Accordingly, in giving the History of Descriptive or Phenomenal Geology, I have called it *Systematic Geology*, just as Classificatory Botany is termed *Systematic Botany*. Moreover, as we have already seen, Classification can never be an arbitrary process, but always implies some natural connexion among the objects of the same Class; for if this connexion did not exist, the Classes could not be made the subjects of any true assertion. Yet though the classes of phenomena which our system acknowledges must be such as already exist in nature, the discovery of these classes is, for the most part, very far from obvious or easy. To detect the true principles of Natural Classes, and to select marks by which these may be recognized, are steps which require genius and good fortune, and which fall to the lot only of the most eminent persons in each science. In the History, I have pointed out Werner, William Smith, and Cuvier, as the three great authors of Systematic Geology of Europe. The mode of classifying the materials of the earth's surface which was found, by these philosophers, fitted to

enunciate such general facts as came under their notice, was to consider the rocks and other materials as divided into successive layers or strata, superimposed one on another, and variously inclined and broken. The German geologist distinguished his strata for the most part by their mineralogical character; the other two, by the remains of animals and plants which the rocks contained. After a beginning had thus been made in giving a genuine scientific form to phenomenal geology, other steps followed in rapid succession, as has already been related in the History[1]. The Classification of the Strata was fixed by a suitable Nomenclature. Attempts were made to apply to other countries the order of strata which had been found to prevail in that first studied: and in this manner it was ascertained what rocks in distant regions are the synonyms, or *Equivalents*[2], of each other. The knowledge thus collected and systematized was exhibited in the form of Geological Maps.

Moreover, among the phenomena of geology we have Laws of Nature as well as Classes. The general form of mountain-chains; the relations of the direction and inclination of different chains to each other; the general features of mineral veins, faults, and fissures; the prevalent characters of slaty cleavage;—were the subjects of laws established, or supposed to be established, by extensive observation of facts. In like manner the organic fossils discovered in the strata were found to follow certain laws with reference to the climate which they appeared to have lived in; and the evidence which they gave of a regular zoological development. And thus, by the assiduous labours of many accomplished and active philosophers, Descriptive or Phenomenal Geology was carried towards a state of completeness.

5. *Phenomenal Uranography.*—In like manner in other palætiological researches, as soon as they approach to an exact and scientific form, we find the necessity of constructing in the first place a science of

[1] *Hist. Ind. Sci.* b. xviii. c. iii. [2] *Ib.* sect. 4.

classification and exact description, by means of which the phenomena may be correctly represented and compared; and of obtaining by this step a solid basis for an inquiry into the causes which have produced them. Thus the Palætiology of the Solar System has, in recent times, drawn the attention of speculators; and a hypothesis has been started, that our sun and his attendant planets have been produced by the condensation of a mass of diffused matter, such as that which constitutes the nebulous patches which we observe in the starry heavens. But the sagest and most enlightened astronomers have not failed to acknowledge, that to verify or to disprove this conjecture, must be the work of many ages of observation and thought. They have perceived also that the first step of the labour requisite for the advancement of this portion of science must be to obtain and to record the most exact knowledge at present within our reach, respecting the phenomena of these nebulæ, with which we thus compare our own system; and, as a necessary element of such knowledge, they have seen the importance of a classification of these objects, and of others, such as Double Stars, of the same kind. Sir William Herschel, who first perceived the bearing of the phenomena of nebulæ upon the history of the solar system, made the observation of such objects his business, with truly admirable zeal and skill; and in the account of the results of his labours, gave a classification of Nebulæ; separating them into, first, *Clusters of Stars;* second, *Resolvable Nebulæ;* third, *Proper Nebulæ;* fourth, *Planetary Nebulæ;* fifth, *Stellar Nebulæ;* sixth, *Nebulous Stars*[3]. And since, in order to obtain from these remote appearances, any probable knowledge respecting our own system, we must discover whether they undergo any changes in the course of ages, he devoted himself to the task of forming a record of their number and appearance in his own time, that thus the astronomers of succeeding generations might have a

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[3] *Phil. Trans.* 1786 and 1789, and Sir J. Herschel's *Astronomy*, Art. 616.

definite and exact standard with which to compare their observations. Still, this task would have been executed only for that part of the heavens which is visible in this country, if this Hipparchus of the Nebulæ and Double Stars had not left behind him a son who inherited all his father's zeal and more than his father's knowledge. Sir John Herschel in 1833 went to the Cape of Good Hope to complete what Sir William Herschel left wanting; and in the course of five years observed with care all the nebulæ and double stars of the Southern hemisphere. This great *Herschelian Survey of the Heavens*, the completion of which is the noblest monument ever erected by a son to a father, must necessarily be, to all ages, the basis of all speculations concerning the history and origin of the solar system; and has completed, so far as at present it can be completed, the phenomenal portion of Astronomical Palætiology.

6. *Phenomenal Geography of Plants and Animals.*—Again, there is another Palætiological Science, closely connected with the speculations forced upon the geologist by the organic fossils which he discovers imbedded in the strata of the earth;—namely, the Science which has for its object the Causes of the Diffusion and Distribution of the various kinds of Plants and Animals. And the science also has for its first portion and indispensable foundation a description and classification of the existing phenomena. Such portions of science have recently been cultivated with great zeal and success, under the titles of the *Geography of Plants*, and the *Geography of Animals.* And the results of the inquiries thus undertaken have assumed a definite and scientific form by leading to a division of the earth's surface into a certain number of botanical and zoological *Provinces*, each province occupied by its own peculiar vegetable and animal population. We find, too, in the course of these investigations, various general laws of the phenomena offered to our notice; such, for instance, as this:—that the difference of the animals originally occupying each province, which is clear and entire for the higher orders of

animals and plants, becomes more doubtful and indistinct when we descend to the lower kinds of organizations; as Infusoria and Zoophytes[4] in the animal kingdom, Grasses and Mosses among vegetables. Again, other laws discovered by those who have studied the geography of plants are these:—that countries separated from each other by wide tracts of sea, as the opposite shores of the Mediterranean, the islands of the Indian and Pacific Oceans, have usually much that is common in their vegetation:—and again, that in parallel climates, analogous tribes replace each other. It would be easy to adduce other laws, but those already stated may serve to show the great extent of the portions of knowledge which have just been mentioned, even considered as merely Sciences of Phenomena.

7. *Phenomenal Glossology.*—It is not my purpose in the present work to borrow my leading illustrations from any portions of knowledge but those which are concerned with the study of material nature; and I shall, therefore, not dwell upon a branch of research, singularly interesting, and closely connected with the one just mentioned, but dealing with relations of thought rather than of things;—I mean the Palætiology of Language;—the theory, so far as the facts enable us to form a theory, of the causes which have led to the resemblances and differences of human speech in various regions and various ages. This, indeed, would be only a portion of the study of the history and origin of the diffusion of animals, if we were to include *man* among the animals whose dispersion we thus investigate; for language is one of the most clear and imperishable records of the early events in the career of the human race. But the peculiar nature of the faculty of speech, and the ideas which the use of it involves, make it proper to treat *Glossology* as a distinct science. And of this science, the first part must necessarily be, as in the other sciences of this order, a

[4] Prichard, *Researches into the Physical History of Mankind*, i. 55, 28.

classification and comparison of languages governed in many respects by the same rules, and presenting the same dfficulties, as other sciences of classification. Such, accordingly, has been the procedure of the most philosophical glossologists. They have been led to throw the languages of the earth into certain large classes or *Families*, according to various kinds of resemblance; as the *Semitic* Family, to which belong Hebrew, Arabic, Chaldean, Syrian, Phœnician, Ethiopian, and the like; the *Indo-European*, which includes Sanskrit, Persian, Greek, Latin, and German; the *Monosyllabic* languages, Chinese, Tibetan, Birman, Siamese; the *Polysynthetic* languages, a class including most of the North-American Indian dialects; and others. And this work of classification has been the result of the labour and study of many very profound linguists, and has advanced gradually from step to step. Thus the Indo-European Family was first formed on an observation of the coincidences between Sanskrit, Greek, and Latin; but it was soon found to include the Teutonic languages, and more recently Dr. Prichard[5] has shown beyond doubt that the Celtic must be included in the same Family. Other general resemblances and differences of languages have been marked by appropriate terms: thus August von Schlegel has denominated them *synthetical* and *analytical*, according as they form their conjugations and declensions by auxiliary verbs and prepositions, or by changes in the word itself: and the *polysynthetic* languages are so named by M. Duponceau, in consequence of their still more complex mode of inflexion. Nor are there wanting, in this science also, general laws of phenomena; such, for instance, is the curious rule of the interchange of consonants in the cognate words of Greek, Gothic, and German, which has been discovered by James Grimm. All these remarkable portions of knowledge, and the great works which have appeared on Glossology, such, for example, as the *Mithridates* of Adelung and Vater, contain, for their largest, and

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[5] Dr Prichard, *On the Eastern Origin of the Celtic Nations.* 1831.

hitherto probably their most valuable part, the phenomenal portion of the science, the comparison of languages as they now are. And beyond all doubt, until we have brought this Comparative Philology to a considerable degree of completeness, all our speculations respecting the causes which have operated to produce the languages of the earth must be idle and unsubstantial dreams.

Thus in all Palætiological Sciences, in all attempts to trace back the history and discover the origin of the present state of things, the portion of the science which must first be formed is that which classifies the phenomena, and discovers general laws prevailing among them. When this work is performed, and not till then, we may begin to speculate successfully concerning causes, and to make some progress in our attempts to go back to an origin. We must have a *Phenomenal* science preparatory to each *Ætiological* one.

8. *The Study of Phenomena leads to Theory.*—As we have just said, we cannot, in any subject, speculate successfully concerning the causes of the present state of things, till we have obtained a tolerably complete and systematic view of the phenomena. Yet in reality men have not in any instance waited for this completeness and system in their knowledge of facts before they have begun to form theories. Nor was it natural, considering the speculative propensities of the human mind, and how incessantly it is endeavouring to apply the Idea of Cause, that it should thus restrain itself. I have already noticed this in the History of Geology. 'While we have been giving an account,' it is there said, 'of the objects with which Descriptive Geology is occupied, it must have been felt how difficult it is, in contemplating such facts, to confine ourselves to description and classification. Conjectures and reasonings respecting the causes of the phenomena force themselves upon us at every step; and even influence our classification and nomenclature. Our Descriptive Geology impels us to construct a Physical Geology.' And the same is the case with regard to the other subjects which I have mentioned. The mere

consideration of the different degrees of condensation of different Nebulæ led Herschel and Laplace to contemplate the hypothesis that our solar system is a condensed Nebula. Immediately upon the division of the earth's surface into botanical and zoological provinces, and even at an earlier period, the opposite hypotheses of the Origin of all the animals of each kind from a single pair, and of their original diffusion all over the earth, were under discussion. And the consideration of the families of languages irresistibly led to speculations concerning the Families of the earliest human inhabitants of the earth. In all cases the contemplation of a very few phenomena, the discovery of a very few steps in the history, made men wish for and attempt to form a theory of the history from the very beginning of things.

9. *No sound Theory without Ætiology.*—But though man is thus impelled by the natural propensities of his intellect to trace each order of things to its causes, he does not at first discern the only sure way of obtaining such knowledge: he does not suspect how much labour and how much method are requisite for success in this undertaking: he is not aware that for each order of phenomena he must construct, by the accumulated results of multiplied observation and distinct thought, a separate Ætiology. Thus, as I have elsewhere remarked[6], when men had for the first time become acquainted with some of the leading phenomena of Geology, and had proceeded to speculate concerning the past changes and revolutions by which such results had been produced, they forthwith supposed themselves able to judge what would be the effects of any of the obvious agents of change, as Water or Volcanic Fire. It did not at first occur to them to suspect that their common and extemporaneous judgment on such points was by no means sufficient for sound knowledge. They did not foresee that, before they could determine what share these or any other causes had had in producing the present condition of the earth, they must create

[6] *Hist. Ind. Sc.* b. xviii. c. v. sect. 1.

a special science whose object should be to estimate the general laws and effects of such assumed causes; —that before they could obtain any sound Geological Theory, they must carefully cultivate Geological Ætiology.

The same disposition to proceed immediately from the facts to the theory, without constructing, as an intermediate step, a Science of Causes, might be pointed out in the other sciences of this order. But in all of them this errour has been corrected by the failures to which it led. It soon appeared, for instance, that a more careful inquiry into the effects which climate, food, habit and circumstances can produce in animals, was requisite in order to determine how the diversities of animals in different countries have originated. The Ætiology of Animal Life (if we may be allowed to give this name to that study of such causes of change which is at present so zealously cultivated, and which yet has no distinctive designation, except so far as it coincides with the *Organic Geological Dynamics* of our *History*) is now perceived to be a necessary portion of all attempts to construct a history of the earth and its inhabitants.

10. *Cause, in Palætiology.*—We are thus led to contemplate a class of Sciences which are commenced with the study of Causes. We have already considered sciences which depended mainly upon the Idea of Cause, namely, the Mechanical Sciences. But it is obvious that the Idea of Cause in the researches now under our consideration must be employed in a very different way from that in which we applied it formerly. Force is the *Cause* of motion, because force at all times and under all circumstances, if not counteracted, produces motion; but the Cause of the present condition and elevation of the Alps, whatever it was, was manifested in a series of events of which each happened but once, and occupied its proper place in the series of time. The former is *mechanical*, the latter *historical*, *cause*. In our present investigations, we consider the events which we contemplate, of whatever order they be, as forming a chain which is extended

from the beginning of things down to the present time; and the causes of which we now speak are those which connect the successive links of this chain. Every occurrence which has taken place in the history of the solar system, or the earth, or its vegetable and animal creation, or man, has been at the same time effect and cause;—the effect of what preceded, the cause of what succeeded. By being effect and cause, it has occupied some certain portion of time; and the times which have thus been occupied by effects and causes, summed up and taken altogether, make up the total of Past Time. The Past has been a series of events connected by this historical causation, and the Present is the last term of this series. The problem in the Palætiological Sciences, with which we are here concerned, is, to determine the manner in which each term is derived from the preceding, and thus, if possible, to calculate backwards to the origin of the series.

11. *Various kinds of Cause.*—Those modes by which one term in the natural series of events is derived from another,—the forms of historical causation,—the kinds of connexion between the links of the infinite chain of time,—are very various; nor need we attempt to enumerate them. But these kinds of causation being distinguished from each other, and separately studied, each becomes the subject of a separate Ætiology. Thus the causes of change in the earth's surface, residing in the elements, fire and water, form the main subject of Geological Ætiology. The Ætiology of the vegetable and animal kingdoms investigates the causes by which the forms and distribution of species of plants and animals are affected. The study of causes in Glossology leads to an Ætiology of Language, which shall distinguish, analyse, and estimate the causes by which certain changes are produced in the languages of nations; in like manner we may expect to have an Ætiology of Art, which shall scrutinise the influences by which the various forms of art have each given birth to its successor: by which, for example, there have been brought into being those various forms of architecture which we term Egyptian,

Doric, Ionic, Roman, Byzantine, Romanesque, Gothic, Italian, Elizabethan. It is easily seen by this slight survey how manifold and diverse are the kinds of cause which the Palætiological Sciences bring under our consideration. But in each of those sciences we shall obtain solid and complete systems of knowledge, only so far as we study, with steady thought and careful observation, that peculiar kind of cause which is appropriate to the phenomena under our consideration.

12. *Hypothetical Order of Palætiological Causes.*—The various kinds of historical cause are not only connected with each other by their common bearing upon the historical sciences, but they form a kind of progression which we may represent to ourselves as having acted *in succession* in the hypothetical history of the earth and its inhabitants. Thus assuming, merely as a momentary hypothesis, the origin of the Solar System by the condensation of a Nebula, we have to contemplate, first, the causes by which the luminous incandescent diffused mass of which a nebula is supposed to be constituted, is gradually condensed, cooled, collected into definite masses, solidified, and each portion made to revolve about its axis, and the whole to travel about another body. We have no difficulty in ascribing the globular form of each mass to the mutual attraction of its particles: but when this form was once assumed, and covered with a solid crust, are there, we may ask, in the constitution of such a body, any causes at work by which the crust might be again broken up and portions of it displaced, and covered with other matter? Again, if we can thus explain the origin of the Earth, can we with like success account for the presence of the Atmosphere and the Waters of earth and ocean? Supposing this done, we have then to consider by what causes such a body could become stocked with vegetable and animal Life; for there have not been wanting persons, extravagant speculators, no doubt, who have conceived that even this event in the history of the world might be the work of natural causes. Supposing an origin given to life

upon our earth, we have then, brought before us by geological observations, a series of different forms of vegetable and animal existence; occurring in different strata, and, as the phenomena appear irresistibly to prove, existing at successive periods: and we are compelled to inquire what can have been the causes by which the forms of each period have passed into those of the next. We find, too, that strata, which must have been at first horizontal and continuous, have undergone enormous dislocations and ruptures, and we have to consider the possible effect of aqueous and volcanic causes to produce such changes in the earth's crust. We are thus led to the causes which have produced the present state of things on the earth; and these are causes to which we may hypothetically ascribe, not only the form and position of the inert materials of the earth, but also the nature and distribution of its animal and vegetable population. Man too, no less than other animals, is affected by the operation of such causes as we have referred to, and must, therefore, be included in such speculations. But man's history only begins, where that of other animals ends, with his mere existence. They are stationary, he is progressive. Other species of animals, once brought into being, continue the same through all ages; man is changing, from age to age, his language, his thoughts, his works. Yet even these changes are bound together by laws of causation; and these causes too may become objects of scientific study. And such causes, though not to be dwelt upon now, since we permit ourselves to found our philosophy upon the material sciences only, must still, when treated scientifically, fall within the principles of our philosophy, and must be governed by the same general rules to which all science is subject. And thus we are led by a close and natural connexion, through a series of causes, extending from those which regulate the imperceptible changes of the remotest nebulæ in the heavens, to those which determine the diversities of language, the mutations of art, and even the progress of civilization, polity, and literature.

While I have been speaking of this supposed series of events, including in its course the formation of the earth, the introduction of animal and vegetable life, and the revolutions by which one collection of species has succeeded another, it must not be forgotten, that though I have thus hypothetically spoken of these events as occurring by force of natural causes, this has been done only that the true efficacy of such causes might be brought under our consideration and made the subject of scientific examination. It may be found, that such occurrences as these are quite inexplicable by the aid of any natural causes with which we are acquainted; and thus, the result of our investigations, conducted with strict regard to scientific principles, may be, that we must either contemplate supernatural influences as part of the past series of events, or declare ourselves altogether unable to form this series into a connected chain.

13. *Mode of Cultivating Ætiology:—In Geology.*— In what manner, it may be asked, is Ætiology, with regard to each subject such as we have enumerated, to be cultivated? In order to answer this question, we must, according to our method of proceeding, take the most successful and complete examples which we possess of such portions of science. But in truth, we can as yet refer to few examples of this kind. In Geology, it is only very recently, and principally through the example and influence of Sir Charles Lyell, that the Ætiology has been detached from the descriptive portion of the science; and cultivated with direct attention: in other sciences the separation has hardly yet been made. But if we examine what has already been done in Geological Ætiology, or as in the *History* it is termed, *Geological Dynamics*, we shall find a number of different kinds of investigation which, by the aid of our general principles respecting the formation of sciences, may suffice to supply very useful suggestions for Ætiology in general.

In Geological Ætiology, causes have been studied, in many instances, by attending to their action in the phenomena of the present state of things, and by inferring

from this the nature and extent of the action which they may have exercised in former times. This has been done, for example, by Von Hoff, Sir Charles Lyell, and others, with regard to the operations of rivers, seas, springs, glaciers, and other aqueous causes of change. Again, the same course has been followed by the same philosophers with respect to volcanoes, earthquakes, and other violent agents. Sir Charles Lyell has attempted to show, too, that there take place, in our own time, not only violent agitations, but slow motions of parts of the earth's crust, of the same kind and order with those which have assisted in producing all anterior changes.

But while we thus seek instruction in the phenomena of the present state of things, we are led to the question, What are the limits of this 'present' period? For instance, among the currents of lava which we trace as part of the shores of Italy and Sicily, *which* shall we select as belonging to the existing order of things? In going backwards in time, where shall we draw the line? and why at such particular point? These questions are important, for our estimate of the efficacy of known causes will vary with the extent of the effects which we ascribe to them. Hence the mode in which we group together rocks is not only a step in geological classification, but is also important to Ætiology. Thus, when the vast masses of trap rocks in the Western Isles of Scotland and in other countries, which had been maintained by the Wernerians to be of aqueous origin, were, principally by the sagacity and industry of Macculloch, identified as to their nature with the products of recent volcanoes, the amount of effect which might justifiably be ascribed to volcanic agency was materially extended.

In other cases, instead of observing the current effects of our geological causes, we have to estimate the results from what we know of the causes themselves; as when, with Herschel, we calculate the alterations in the temperature of the earth which astronomical changes may possibly produce; or when, with Fourier, we try to calculate the rate of cooling of the earth's

surface, on the hypothesis of an incandescent central mass. In other cases, again, we are not able to calculate the effects of our causes rigorously, but estimate them as well as we can; partly by physical reasonings, and partly by comparison with such analogous cases as we can find in the present state of things. Thus Sir Charles Lyell infers the change of climate which would result if land were transferred from the neighbourhood of the poles to that of the equator, by reasonings on the power of land and water to contain and communicate heat, supported by a reference to the different actual climates of places, lying under the same latitude, but under different conditions as to the distribution of land and water.

Thus our Ætiology is constructed partly from calculation and reasoning, partly from phenomena. But we may observe that when we reason from phenomena to causes, we usually do so by various steps; often ascending from phenomena to mere laws of phenomena, before we can venture to connect the phenomenon confidently with its cause. Thus the law of subterranean heat, that it increases in descending below the surface, is now well established, although the doctrine which ascribes this effect to a central heat is not universally assented to.

14. *In the Geography of Plants and Animals.*—We may find in other subjects also, considerable contributions towards Ætiology, though not as yet a complete System of Science. The Ætiology of Vegetables and Animals, indeed, has been studied with great zeal in modern times, as an essential preparative to geological theory; for how can we decide whether any assumed causes have produced the succession of species which we find in the earth's strata, except we know what effect of this kind given causes can produce? Accordingly, we find in Sir Charles Lyell's *Treatise on Geology* the most complete discussion of such questions as belong to these subjects:—for example, the question whether species can be transmuted into other species by the long-continued influence of external causes, as climate, food, domestication, combined with internal

causes, as habits, appetencies, progressive tendencies. We may observe, too, that as we have, brought before us, the inquiry what change difference of climate can produce in any species, we have also the inverse problem, how far a different development of the species, or a different collection of species, proves a difference of climate. In the same way, the geologist of the present day considers the question, whether, in virtue of causes now in action, species are from time to time extinguished; and in like manner, the geologists of an earlier period discussed the question, now long completely decided, whether fossil species in general are really extinct species.

15. *In Languages.*—Even with reference to the Ætiology of Language, although this branch of science has hardly been considered separately from the glossological investigations in which it is employed or assumed to be employed, it might perhaps be possible to point out causes or conditions of change which, being general in their nature, must operate upon all languages alike. Changes made for the sake of euphony when words are modified and combined, occur in all dialects. Who can doubt that such changes of consonants as those by which the Greek roots become Gothic, and the Gothic, German, have for their cause some general principle in the pronunciation of each language? Again, we might attempt to decide other questions of no small interest. Have the terminations of verbs arisen from the accretion of pronouns; or, on the other hand, does the modification of a verb imply a simpler mental process than the insulation of a pronoun, as Adam Smith has maintained? Again, when the language of a nation is changed by the invasion and permanent mixture of an enemy of different speech, is it generally true that it is changed from a synthetic to an analytical structure? I will mention only one more of these wide and general glossological inquiries. Is it true, as Dr. Prichard has suggested[7], that languages have become more permanent as we come down

[7] *Researches*, ii. 221.

towards later times? May we justifiably suppose, with him, that in the very earliest times, nations, when they had separated from one stock, might lose all traces of this common origin out of their languages, though retaining strong evidences of it in their mythology, social forms, and arts, as appears to be the case with the ancient Egyptians and the Indians[8].

Large questions of this nature cannot be treated profitably in any other way than by an assiduous study of the most varied forms of living and dead languages. But on the other hand, the study of languages should be prosecuted not only by a direct comparison of one with another, but also with a view to the formation of a science of causes and general principles, embracing such discussions as I have pointed out. It is only when such a science has been formed, that we can hope to obtain any solid and certain results in the Palætiology of Language;—to determine, with any degree of substantial proof, what is the real evidence which the wonderful faculty of speech, under its present developments and forms, bears to the events which have taken place in its own history, and in the history of man since his first origin.

16. *Construction of Theories.*—When we have thus obtained, with reference to any such subject as those we have here spoken of, these two portions of science, a Systematic Description of the Facts, and a rigorous Analysis of the Causes,—the *Phenomenology* and the *Ætiology* of the subject,—we are prepared for the third member which completes the science, the *Theory* of the actual facts. We can then take a view of the events which really have happened, discerning their connexion, interpreting their evidence, supplying from the context the parts which are unapparent. We can account for known facts by intelligible causes; we can infer latent facts from manifest effects, so as to obtain a distinct insight into the whole history of events up to the present time, and to see the last result of the whole in the present condition of things.

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[8] *Researches*, ii. 192.

The term *Theory*, when rigorously employed in such sciences as those which we here consider, bears nearly the sense which I have adopted: it implies a consistent and systematic view of the actual facts, combined with a true apprehension of their connexion and causes. Thus if we speak of 'a Theory of Mount Etna,' or 'a Theory of the Paris Basin,' we mean a connected and intelligible view of the events by which the rocks in these localities have come into their present condition. Undoubtedly the term *Theory* has often been used in a looser sense; and men have put forth '*Theories of the Earth*,' which, instead of including the whole mass of actual geological facts and their causes, only assigned, in a vague manner, some causes by which some few phenomena might, it was conceived, be accounted for. Perhaps the portion of our Palætiological Sciences which we now wish to designate, would be more generally understood if we were to describe it as *Theoretical* or *Philosophical History;* as when we talk of 'the Theoretical History of Architecture,' or 'the Philosophical History of Language.' And in the same manner we might speak of the Theoretical History of the Animal and Vegetable Kingdoms; meaning, a distinct account of the events which have produced the present distribution of species and families. But by whatever phrase we describe this portion of science, it is plain that such a Theory, such a Theoretical History, must result from the application of causes well understood to facts well ascertained. And if the term *Theory* be here employed, we must recollect that it is to be understood, not in its narrower sense as opposed to facts, but in its wider signification, as including all known facts and differing from them only in introducing among them principles of intelligible connexion. The Theories of which we now speak are true *Theories*, precisely because they are identical with the total system of the *Facts*.

17. *No sound Palætiological Theory yet extant.*— It is not to disparage unjustly the present state of science, to say that as yet no such theory exists on any subject. 'Theories of the Earth' have been re-

peatedly published; but when we consider that even the facts of geology have been observed only on a small portion of the earth's surface, and even within those narrow bounds very imperfectly studied, we shall be able to judge how impossible it is that geologists should have yet obtained a well-established Theoretical History of the changes which have taken place in the crust of the terrestrial globe from its first origin. Accordingly, I have ventured in my History to designate the most prominent of the Theories which have hitherto prevailed as *premature* geological theories[9]: and we shall soon see that geological theory has not advanced beyond a few conjectures, and that its cultivators are at present mainly occupied with a controversy in which the two extreme hypotheses which first offer themselves to men's minds are opposed to each other. And if we have no theoretical History of the Earth which merits any confidence, still less have we any theoretical History of Language, or of the Arts, which we can consider as satisfactory. The Theoretical History of the Vegetable and Animal Kingdoms is closely connected with that of the Earth on which they subsist, and must follow the fortunes of Geology. And thus we may venture to say that no Palætiological Science, as yet, possesses all its three members. Indeed most of them are very far from having completed and systematized their Phenomenology: in all, the cultivation of Ætiology is but just begun, or is not begun; in all, the Theory must reward the exertions of future, probably of distant, generations.

But in the mean time we may derive some instruction from the comparison of the two antagonist hypotheses of which I have spoken.

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[9] *Hist. Ind. Sc.* b. xviii. c. vii. sect. 3.

CHAPTER III.

OF THE DOCTRINE OF CATASTROPHES AND THE DOCTRINE OF UNIFORMITY.

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1. *Doctrine of Catastrophes.*—I HAVE already shown, in the History of Geology, that the attempts to frame a theory of the earth have brought into view two completely opposite opinions:—one, which represents the course of nature as *uniform* through all ages, the causes which produce change having had the same intensity in former times which they have at the present day;—the other opinion, which sees, in the present condition of things, evidences of *catastrophes;*—changes of a more sweeping kind, and produced by more powerful agencies than those which occur in recent times. Geologists who held the latter opinion, maintained that the forces which have elevated the Alps or the Andes to their present height could not have been any forces which are now in action: they pointed to vast masses of strata hundreds of miles long, thousands of feet thick, thrown into highly-inclined positions, fractured, dislocated, crushed: they remarked that upon the shattered edges of such strata they found enormous accumulations of fragments and rubbish, rounded by the action of water, so as to denote ages of violent aqueous action: they conceived that they saw instances in which whole mountains of rock in a state of igneous fusion, must have burst the earth's crust from below: they found that in the course of the revolutions by which one stratum of rock was placed upon another, the whole collection of animal species which tenanted the earth and the seas had been removed, and a new set of living things introduced in its place: finally, they found, above all the strata,

vast masses of sand and gravel containing bones of animals, and apparently the work of a mighty deluge. With all these proofs before their eyes, they thought it impossible not to judge that the agents of change by which the world was urged from one condition to another till it reached its present state must have been more violent, more powerful, than any which we see at work around us. They conceived that the evidence of 'catastrophes' was irresistible.

2. *Doctrine of Uniformity.*—I need not here repeat the narrative (given in the History[1]) of the process by which this formidable array of proofs was, in the minds of some eminent geologists, weakened, and at last overcome. This was done by showing that the sudden breaks in the succession of strata were apparent only, the discontinuity of the series which occurred in one country being removed by terms interposed in another locality:—by urging that the total effect produced by existing causes, taking into account the accumulated result of long periods, is far greater than a casual speculator would think possible: —by making it appear that there are in many parts of the world evidences of a slow and imperceptible rising of the land since it was the habitation of now existing species:—by proving that it is not universally true that the strata separated in time by supposed catastrophes contain distinct species of animals:—by pointing out the limited fields of the supposed diluvial action:—and finally, by remarking that though the *creation* of species is a mystery, the *extinction* of species is going on in our own day. Hypotheses were suggested, too, by which it was conceived that the change of climate might be explained, which, as the consideration of the fossil remains seemed to show, must have taken place between the ancient and the modern times. In this manner the whole evidence of catastrophes was explained away: the notion of a series of paroxysms of violence in the causes of change was represented as a delusion arising from our contem-

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[1] *Hist. Ind. Sc.* b. xviii. c. viii. sect. 2.

plating short periods only, in the action of present causes: length of time was called in to take the place of intensity of force: and it was declared that Geology need not despair of accounting for the revolutions of the earth, as Astronomy accounts for the revolutions of the heavens, by the universal action of causes which are close at hand to us, operating through time and space without variation or decay.

An antagonism of opinions, somewhat of the same kind as this, will be found to manifest itself in the other Palætiological Sciences as well as in Geology; and it will be instructive to endeavour to balance these opposite doctrines. I will mention some of the considerations which bear upon the subject in its general form.

3. *Is Uniformity probable à priori?*—The doctrine of Uniformity in the course of nature has sometimes been represented by its adherents as possessing a great degree of *à priori* probability. It is highly unphilosophical, it has been urged, to assume that the causes of the geological events of former times were of a different kind from causes now in action, if causes of this latter kind can in any way be made to explain the facts. The analogy of all other sciences compels us, it was said, to explain phenomena by known, not by unknown, causes. And on these grounds the geological teacher recommended[2] 'an earnest and patient endeavour to reconcile the indications of former change with the evidence of gradual mutations now in progress.'

But on this we may remark, that if by *known* causes we mean causes acting with the same intensity which they have had during historical times, the restriction is altogether arbitrary and groundless. Let it be granted, for instance, that many parts of the earth's surface are now undergoing an imperceptible rise. It is not pretended that the rate of this elevation is rigorously uniform; what, then, are the limits of its velocity? Why may it not increase so as to assume that character of violence which we may term a

[2] Lyell, 4th ed. b. iv. c. i. p. 328.

catastrophe with reference to all changes hitherto recorded? Why may not the rate of elevation be such that we may conceive the strata to assume *suddenly* a position nearly vertical? And is it, in fact, easy to conceive a position of strata nearly vertical, a position which occurs so frequently, to be *gradually* assumed? In cases where the strata are nearly vertical, as in the Isle of Wight, and hundreds of other places, or where they are actually inverted, as sometimes occurs, are not the causes which have produced the effect as truly *known* causes, as those which have raised the coasts where we trace the former beach in an elevated terrace? If the latter case proves *slow* elevation, does not the former case prove *rapid* elevation? In neither case have we any measure of the time employed in the change; but does not the very nature of the results enable us to discern, that if one was gradual, the other was comparatively sudden?

The causes which are now elevating a portion of Scandinavia can be called known *causes*, only because we know the *effect*. Are not the causes which have elevated the Alps and the Andes known causes in the same sense? We know nothing in either case which confines the intensity of the force within any limit, or prescribes to it any law of uniformity. Why, then, should we make a merit of cramping our speculations by such assumptions? Whether the causes of change do act uniformly;—whether they oscillate only within narrow limits;—whether their intensity in former times was nearly the same as it now is;—these are precisely the questions which we wish Science to answer to us impartially and truly: where is then the wisdom of 'an earnest and patient endeavour' to secure an *affirmative* reply?

Thus I conceive that the assertion of an *à priori* claim to probability and philosophical spirit in favour of the doctrine of uniformity, is quite untenable. We must learn from an examination of all the facts, and not from any assumption of our own, whether the course of nature be uniform. The limit of intensity being really unknown, catastrophes are just as probable

as uniformity. If a volcano may repose for a thousand years, and then break out and destroy a city; why may not another volcano repose for ten thousand years, and then destroy a continent; or if a continent, why not the whole habitable surface of the earth?

4. *Cycle of Uniformity indefinite.*—But this argument may be put in another form. When it is said that the course of nature is uniform, the assertion is not intended to exclude certain smaller variations of violence and rest, such as we have just spoken of;—alternations of activity and repose in volcanoes; or earthquakes, deluges, and storms, interposed in a more tranquil state of things. With regard to such occurrences, terrible as they appear at the time, they may not much affect the average rate of change; there may be a *cycle*, though an irregular one, of rapid and slow change; and if such cycles go on succeeding each other, we may still call the order of nature uniform, notwithstanding the periods of violence which it involves. The maximum and minimum intensities of the forces of mutation alternate with one another; and we may estimate the average course of nature as that which corresponds to something between the two extremes.

But if we thus attempt to maintain the uniformity of nature by representing it as a series of *cycles*, we find that we cannot discover, in this conception, any solid ground for excluding catastrophes. What is the length of that cycle, the repetition of which constitutes uniformity? What interval from the maximum to the minimum does it admit of? We may take for our cycle a hundred or a thousand years, but evidently such a proceeding is altogether arbitrary. We may mark our cycles by the greatest known paroxysms of volcanic and terremotive agency, but this procedure is no less indefinite and inconclusive than the other.

But further; since the cycle in which violence and repose alternate is thus indefinite in its length and in its range of activity, what ground have we for assuming more than *one* such cycle, extending from the origin of things to the present time? Why may we not suppose the maximum force of the causes of change

to have taken place at the earliest period, and the tendency towards the minimum to have gone on ever since? Or instead of only one cycle, there may have been several, but of such length that our historical period forms a portion only of the last;—the feeblest portion of the latest cycle. And thus violence and repose may alternate upon a scale of time and intensity so large, that man's experience supplies no evidence enabling him to estimate the amount. The course of things is *uniform*, to an Intelligence which can embrace the succession of several cycles, but it is *catastrophic* to the contemplation of man, whose survey can grasp a part only of one cycle. And thus the hypothesis of uniformity, since it cannot exclude degrees of change, nor limit the range of these degrees, nor define the interval of their recurrence, cannot possess any essential simplicity which, previous to inquiry, gives it a claim upon our assent superior to that of the opposite catastrophic hypothesis.

5. *Uniformitarian Arguments are Negative only.*— There is an opposite tendency in the mode of maintaining the catastrophist and the uniformitarian opinions, which depends upon their fundamental principles, and shows itself in all the controversies between them. The Catastrophist is affirmative, the Uniformitarian is negative in his assertions: the former is constantly attempting to construct a theory; the latter delights in demolishing all theories. The one is constantly bringing fresh evidence of some great past event, or series of events, of a striking and definite kind; his antagonist is at every step explaining away the evidence, and showing that it proves nothing. One geologist adduces his proofs of a vast universal deluge; but another endeavours to show that the proofs do not establish either the universality or the vastness of such an event. The inclined broken edges of a certain formation, covered with their own fragments, beneath superjacent horizontal deposits, are at one time supposed to prove a catastrophic breaking up of the earlier strata; but this opinion is controverted by showing that the same formations, when pursued into other countries,

exhibit a uniform gradation from the lower to the upper, with no trace of violence. Extensive and lofty elevations of the coast, continents of igneous rock, at first appear to indicate operations far more gigantic than those which now occur; but attempts are soon made to show that time only is wanting to enable the present age to rival the past in the production of such changes. Each new fact adduced by the catastrophist is at first striking and apparently convincing; but as it becomes familiar, it strikes the imagination less powerfully; and the uniformitarian, constantly labouring to produce some imitation of it by the machinery which he has so well studied, at last in every case seems to himself to succeed, so far as to destroy the effect of his opponent's evidence.

This is so with regard to more remote, as well as with regard to immediate evidences of change. When it is ascertained that in every part of the earth's crust the temperature increases as we descend below the surface, at first this fact seems to indicate a central heat: and a central heat naturally suggests an earlier state of the mass, in which it was incandescent, and from which it is now cooling. But this original incandescence of the globe of the earth is manifestly an entire violation of the present course of things; it belongs to the catastrophist view, and the advocates of uniformity have to explain it away. Accordingly, one of them holds that this increase of heat in descending below the surface may very possibly not go on all the way to the center. The heat which increases at first as we descend, may, he conceives, afterwards decrease; and he suggests causes which may have produced such a succession of hotter and colder shells within the mass of the earth. I have mentioned this suggestion in the History of Geology; and have given my reasons for believing it altogether untenable[3]. Other persons also, desirous of reconciling this subterraneous heat with the tenet of uniformity, have

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[3] *Hist. Ind. Sc.* b. xviii. c. v. sect. 5, and note.

offered another suggestion:—that the warmth or incandescence of the interior parts of the earth does not arise out of an originally hot condition from which it is gradually cooling, but results from chemical action constantly going on among the materials of the earth's substance. And thus new attempts are perpetually making, to escape from the cogency of the reasonings which send us towards an original state of things different from the present. Those who theorize concerning an origin go on building up the fabric of their speculations, while those who think such theories unphilosophical, ever and anon dig away the foundation of this structure. As we have already said, the uniformitarian's doctrines are a collection of negatives.

This is so entirely the case, that the uniformitarian would for the most part shrink from maintaining as positive tenets the explanations which he so willingly uses as instruments of controversy. He puts forward his suggestions as difficulties, but he will not stand by them as doctrines. And this is in accordance with his general tendency; for any of his hypotheses, if insisted upon as positive theories, would be found inconsistent with the assertion of uniformity. For example, the nebular hypothesis appears to give to the history of the heavens an aspect which obliterates all special acts of creation, for, according to that hypothesis, new planetary systems are constantly forming; but when asserted as the origin of our own solar system, it brings with it an original incandescence, and an origin of the organic world. And if, instead of using the chemical theory of subterraneous heat to neutralize the evidence of original incandescence, we assert it as a positive tenet, we can no longer maintain the infinite past duration of the earth; for chemical forces, as well as mechanical, tend to equilibrium; and that condition once attained, their efficacy ceases. Chemical affinities tend to form new compounds; and though, when many and various elements are mingled together, the play of synthesis and analysis may go on for a long time, it must at last end. If, for instance, a large portion of the earth's mass were originally pure potassium, we

can imagine violent igneous action to go on so long as any part remained unoxidized; but when the oxidation of the whole has once taken place, this action must be at an end; for there is in the hypothesis no agency which can reproduce the deoxidized metal. Thus a perpetual motion is impossible in chemistry, as it is in mechanics; and a theory of constant change continued through infinite time, is untenable when asserted upon chemical, no less than upon mechanical principles. And thus the Skepticism of the uniformitarian is of force only so long as it is employed against the Dogmatism of the catastrophist. When the Doubts are erected into Dogmas, they are no longer consistent with the tenet of Uniformity. When the Negations become Affirmations, the Negation of an Origin vanishes also.

6. *Uniformity in the Organic World.*—In speaking of the violent and sudden changes which constitute catastrophes, our thoughts naturally turn at first to great *mechanical* and *physical* effects;—ruptures and displacements of strata; extensive submersions and emersions of land; rapid changes of temperature. But the catastrophes which we have to consider in geology affect the *organic* as well as the inorganic world. The sudden extinction of one collection of species, and the introduction of another in their place, is a Catastrophe, even if unaccompanied by mechanical violence. Accordingly, the antagonism of the catastrophist and uniformitarian schools has shown itself in this department of the subject, as well as in the other. When geologists had first discovered that the successive strata are each distinguished by appropriate organic fossils, they assumed at once that each of these collections of living things belonged to a separate creation. But this conclusion, as I have already said, Sir C. Lyell has attempted to invalidate, by proving that in the existing order of things, some species become extinct; and by suggesting it as possible, that in the same order, it may be true that new species are from time to time produced, even in the present course of nature. And in this, as in the other part of the subject, he calls in

the aid of vast periods of time, in order that the violence of the changes may be softened down: and he appears disposed to believe that the actual extinction and creation of species may be so slow as to excite no more notice than it has hitherto obtained; and yet may be rapid enough, considering the immensity of geological periods, to produce such a succession of different collections of species as we find in the strata of the earth's surface.

7. *Origin of the present Organic World.*—The last great event in the history of the vegetable and animal kingdoms was that by which their various tribes were placed in their present seats. And we may form various hypotheses with regard to the sudden or gradual manner in which we may suppose this distribution to have taken place. We may assume that at the beginning of the present order of things, a stock of each species was placed in the vegetable or animal *province* to which it belongs, by some cause out of the common order of nature; or we may take a uniformitarian view of the subject, and suppose that the provinces of the organic world derived their population from some anterior state of things by the operation of natural causes.

Nothing has been pointed out in the existing order of things which has any analogy or resemblance, of any valid kind, to that creative energy which must be exerted in the production of a new species. And to assume the introduction of new species as 'a part of the order of nature,' without pointing out any natural fact with which such an event can be classed, would be to reject creation by an arbitrary act. Hence, even on natural grounds, the most intelligible view of the history of the animal and vegetable kingdoms seems to be, that each period which is marked by a distinct collection of species forms a cycle; and that at the beginning of each such cycle a creative power was exerted, of a kind to which there was nothing at all analogous in the succeeding part of the same cycle. If it be urged that in some cases the same species, or the same genus, runs through two geological formations,

which must, on other grounds, be referred to different cycles of creative energy, we may reply that the creation of many new species does not imply the extinction of all the old ones.

Thus we are led by our reasonings to this view, that the present order of things was commenced by an act of creative power entirely different to any agency which has been exerted since. None of the influences which have modified the present races of animals and plants since they were placed in their habitations on the earth's surface can have had any efficacy in producing them at first. We are necessarily driven to assume, as the beginning of the present cycle of organic nature, an event not included in the course of nature. And we may remark that this necessity is the more cogent, precisely because other cycles have preceded the present.

8. *Nebular Origin of the Solar System.*—If we attempt to apply the same antithesis of opinion (the doctrines of Catastrophe and Uniformity) to the other subjects of palætiological sciences, we shall be led to similar conclusions. Thus, if we turn our attention to Astronomical Palætiology, we perceive that the Nebular Hypothesis has a uniformitarian tendency. According to this hypothesis the formation of this our system of sun, planets, and satellites, was a process of the same kind as those which are still going on in the heavens. One after another, nebulæ condense into separate masses, which begin to revolve about each other by mechanical necessity, and form systems of which our solar system is a finished example. But we may remark, that the uniformitarian doctrine on this subject rests on most unstable foundations. We have as yet only very vague and imperfect reasonings to show that by such condensation a *material* system such as ours could result; and the introduction of *organized* beings into such a material system is utterly out of the reach of our philosophy. Here again, therefore, we are led to regard the present order of the world as pointing towards an origin altogether of a different kind from anything which our material science can grasp.

9. *Origin of Languages.*—We may venture to say that we should be led to the same conclusion once more, if we were to take into our consideration those palætiological sciences which are beyond the domain of matter; for instance, the History of Languages. We may explain many of the differences and changes which we become acquainted with, by referring to the action of causes of change which still operate. But what glossologist will venture to declare that the efficacy of such causes has been uniform;—that the influences which mould a language, or make one language differ from others of the same stock, operated formerly with no more efficacy than they exercise now. 'Where,' as has elsewhere been asked, 'do we now find a language in the process of formation, unfolding itself in inflexions, terminations, changes of vowels by grammatical relations, such as characterise the oldest known languages?' Again, as another proof how little the history of languages suggests to the philosophical glossologist the persuasion of a uniform action of the causes of change, I may refer to the conjecture of Dr. Prichard, that the varieties of language produced by the separation of one stock into several, have been greater and greater as we go backwards in history:—that[4] the formation of sister dialects from a common language (as the Scandinavian, German, and Saxon dialects from the Teutonic, or the Gaelic, Erse and Welsh from the Celtic) belongs to the first millennium before the Christian era; while the formation of cognate languages of the same family, as the Sanskrit, Latin, Greek and Gothic, must be placed at least two thousand years before that era; and at a still earlier period took place the separation of the great families themselves, the Indo-European, Semitic, and others, in which it is now difficult to trace the features of a common origin. No hypothesis except one of this kind will explain the existence of the families, groups, and dialects of languages, which we find in existence. Yet this is an entirely different view from that which

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[4] *Researches*, ii. 224.

the hypothesis of the uniform progress of change would give. And thus, in the earliest stages of man's career, the revolutions of language must have been, even by the evidence of the theoretical history of language itself, of an order altogether different from any which have taken place within the recent history of man. And we may add, that as the early stages of the progress of language must have been widely different from those later ones of which we can in some measure trace the natural causes, we cannot place the origin of language in any point of view in which it comes under the jurisdiction of natural causation at all.

10. *No Natural Origin discoverable.*—We are thus led by a survey of several of the palætiological sciences to a confirmation of the principle formerly asserted[5], That in no palætiological science has man been able to arrive at a beginning which is homogeneous with the known course of events. We can in such sciences often go very far back;—determine many of the remote circumstances of the past series of events;—ascend to a point which seems to be near the origin;—and limit the hypotheses respecting the origin itself: but philosophers never have demonstrated, and, so far as we can judge, probably never will be able to demonstrate, what was that primitive state of things from which the progressive course of the world took its first departure. In all these paths of research, when we travel far backwards, the aspect of the earlier portions becomes very different from that of the advanced part on which we now stand; but in all cases the path is lost in obscurity as it is traced backwards towards its starting-point: it becomes not only invisible, but unimaginable; it is not only an interruption, but an abyss, which interposes itself between us and any intelligible beginning of things.

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[5] *Hist. Ind. Sc.* b. xviii. c. vi. sect 5.

CHAPTER IV.

Of the Relation of Tradition to Palætiology.

1. *Importance of Tradition.*—Since the Palætiological Sciences have it for their business to study the train of past events produced by natural causes down to the present time, the knowledge concerning such events which is supplied by the remembrance and records of man, in whatever form, must have an important bearing upon these sciences. All changes in the condition and extent of land and sea, which have taken place within man's observation, all effects of deluges, sea-waves, rivers, springs, volcanoes, earthquakes, and the like, which come within the reach of human history, have a strong interest for the palætiologist. Nor is he less concerned in all recorded instances of the modification of the forms and habits of plants and animals, by the operations of man, or by transfer from one land to another. And when we come to the Palætiology of Language, of Art, of Civilization, we find our subject still more closely connected with history; for in truth these are historical, no less than palætiological investigations. But, confining ourselves at present to the material sciences, we may observe that though the importance of the information which tradition gives us, in the sciences now under our consideration, as, for instance, geology, has long been tacitly recognised; yet it is only recently that geologists have employed themselves in collecting their historical facts upon such a scale and with such comprehensive views as are required by the interest and use of collections of this kind. The Essay of Von

Hoff[1], *On the Natural Alterations in the Surface of the Earth which are proved by Tradition*, was the work which first opened the eyes of geologists to the extent and importance of this kind of investigation. Since that time the same path of research has been pursued with great perseverance by others, especially by Sir C. Lyell; and is now justly considered as an essential portion of Geology.

2. *Connexion of Tradition and Science.*—Events which we might naturally expect to have some bearing on geology, are narrated in the historical writings which, even on mere human grounds, have the strongest claim to our respect as records of the early history of the world, and are confirmed by the traditions of various nations all over the globe; namely, the formation of the earth and of its population, and a subsequent deluge. It has been made a matter of controversy how the narrative of these events is to be understood, so as to make it agree with the facts which an examination of the earth's surface and of its vegetable and animal population discloses to us. Such controversies, when they are considered as merely archæological, may occur in any of the palætiological sciences. We may have to compare and to reconcile the evidence of existing phenomena with that of historical tradition. But under some circumstances this process of conciliation may assume an interest of another kind, on which we will make a few remarks.

3. *Natural and Providential History of the World.*—We may contemplate the existence of man upon the earth, his origin and his progress, in the same manner as we contemplate the existence of any other race of animals; namely, in a purely palætiological view. We may consider how far our knowledge of laws of causation enables us to explain his diffusion and migration, his differences and resemblances, his actions and works. And this is the view of man as a member of the *Natural* Course of Things.

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[1] Vol. i. 1822; vol. ii. 1824.

But man, at the same time the contemplator and the subject of his own contemplation, endowed with faculties and powers which make him a being of a different nature from other animals, cannot help regarding his own actions and enjoyments, his recollections and his hopes, under an aspect quite different from any that we have yet had presented to us. We have been endeavouring to place in a clear light the Fundamental Ideas, such as that of Cause, on which depends our knowledge of the natural course of things. But there are other Ideas to which man necessarily refers his actions; he is led by his nature, not only to consider his own actions, and those of his fellow-men, as springing out of this or that cause, leading to this or that material result; but also as *good* or *bad*, as what they *ought* or *ought not* to be. He has Ideas of *moral* relations as well as those Ideas of material relations with which we have hitherto been occupied. He is a moral as well as a natural agent.

Contemplating himself and the world around him by the light of his Moral Ideas, man is led to the conviction that his moral faculties were bestowed upon him by design and for a purpose; that he is the subject of a Moral Government; that the course of the world is directed by the Power which governs it, to the unfolding and perfecting of man's moral nature; that this guidance may be traced in the career of individuals and of the world; that there is a *Providential* as well as a Natural Course of Things.

Yet this view is beset by no small difficulties. The full development of man's moral faculties;—the perfection of his nature up to the measure of his own ideas;—the adaptation of his moral being to an ultimate destination, by its transit through a world full of moral evil, in which evil each person has his share;—are effects for which the economy of the world appears to contain no adequate provision. Man, though aware of his moral nature, and ready to believe in an ultimate destination of purity and blessedness, is too feeble to resist the temptation of evil, and too helpless to restore his purity when once lost. He cannot but look for

some confirmation of that providential order which he has begun to believe; some provision for those deficiencies in his moral condition which he has begun to feel.

He looks at the history of the world, and he finds that at a certain period it offers to him the promise of what he seeks. When the natural powers of man had been developed to their full extent, and were beginning to exhibit symptoms of decay;—when the intellectual progress of the world appeared to have reached its limit, without supplying man's moral needs;—we find the great Epoch in the Providential History of the world. We find the announcement of a Dispensation by which man's deficiencies shall be supplied and his aspirations fulfilled: we find a provision for the purification, the support, and the ultimate beatification of those who use the provided means. And thus the providential course of the world becomes consistent and intelligible.

4. *The Sacred Narrative.*—But with the new Dispensation, we receive, not only an account of its own scheme and history, but also a written narrative of the providential course of the world from the earliest times, and even from its first creation. This narrative is recognized and authorized by the new dispensation, and accredited by some of the same evidences as the dispensation itself. That the existence of such a sacred narrative should be a part of the providential order of things, cannot but appear natural; but, naturally also, the study of it leads to some difficulties.

The Sacred Narrative in some of its earliest portions speaks of natural objects and occurrences respecting them. In the very beginning of the course of the world, we may readily believe (indeed, as we have seen in the last chapter, our scientific researches lead us to believe) that such occurrences were very different from anything which now takes place;—different to an extent and in a manner which we cannot estimate. Now the narrative must speak of objects and occurrences in the words and phrases which have derived their meaning from their application to the existing natural state of things. When applied to an initial

supernatural state therefore, these words and phrases cannot help being to us obscure and mysterious, perhaps ambiguous and seemingly contradictory.

5. *Difficulties in interpreting the Sacred Narrative.*—The moral and providential relations of man's condition are so much more important to him than mere natural relations, that at first we may well suppose he will accept the Sacred Narrative, as not only unquestionable in its true import, but also as a guide in his views even of mere natural things. He will try to modify the conceptions which he entertains of objects and their properties, so that the Sacred Narrative of the supernatural condition shall retain the first meaning which he had put upon it in virtue of his own habits in the usage of language.

But man is so constituted that he cannot persist in this procedure. The powers and tendencies of his intellect are such that he cannot help trying to attain true conceptions of objects and their properties by the study of things themselves. For instance, when he at first read of a firmament dividing the waters above from the waters below, he perhaps conceived a transparent floor in the skies, on which the superior waters rested, which descend in rain; but as his observations and his reasonings satisfied him that such a floor could not exist, he became willing to allow (as St. Augustine allowed) that the waters above the firmament are in a state of vapour. And in like manner in other subjects, men, as their views of nature became more distinct and precise, modified, so far as it was necessary for consistency's sake, their first rude interpretations of the Sacred Narrative; so that, without in any degree losing its import as a view of the providential course of the world, it should be so conceived as not to contradict what they knew of the natural order of things.

But this accommodation was not always made without painful struggles and angry controversies. When men had conceived the occurrences of the Sacred Narrative in a particular manner, they could not readily and willingly adopt a new mode of conception; and all attempts to recommend to them such novelties, they

resisted as attacks upon the sacredness of the Narrative. They had clothed their belief of the workings of Providence in certain images; and they clung to those images with the persuasion that, without them, their belief could not subsist. Thus they imagined to themselves that the earth was a flat floor, solidly and broadly laid for the convenience of man; and they felt as if the kindness of Providence was disparaged, when it was maintained that the earth was a globe held together only by the mutual attraction of its parts.

The most memorable instance of a struggle of this kind is to be found in the circumstances which attended the introduction of the Heliocentric Theory of Copernicus to general acceptance. On this controversy I have already made some remarks in the *History of Science*[2], and have attempted to draw from it some lessons which may be useful to us when any similar conflict of opinions may occur. I will here add a few reflections with a similar view.

6. *Such difficulties inevitable.*—In the first place, I remark that such modifications of the current interpretation of the words of Scripture appear to be an inevitable consequence of the progressive character of Natural Science. Science is constantly teaching us to describe known facts in new language; but the language of Scripture is always the same. And not only so, but the language of Scripture is necessarily adapted to the common state of man's intellectual development, in which he is supposed not to be possessed of science. Hence the phrases used by Scripture are precisely those which science soon teaches man to consider as inaccurate. Yet they are not, on that account, the less fitted for their proper purpose: for if any terms had been used, adapted to a more advanced state of knowledge, they must have been unintelligible among those to whom the Scripture was first addressed. If the Jews had been told that water existed in the clouds in small drops, they would have marvelled that it did

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[2] B. v. c. iii. sect. 4.

not constantly descend; and to have explained the reason of this, would have been to teach Atmology in the sacred writings. If they had read in their Scripture that the earth was a sphere, when it appeared to be a plain, they would only have been disturbed in their thoughts or driven to some wild and baseless imaginations, by a declaration to them so strange. If the Divine Speaker, instead of saying that he would set his bow in the clouds, had been made to declare that he would give to water the property of refracting different colours at different angles, how utterly unmeaning to the hearers would the words have been! And in these cases, the expressions, being unintelligible, startling, and bewildering, would have been such as tended to unfit the Sacred Narrative for its place in the providential dispensation of the world.

Accordingly, in the great controversy which took place in Galileo's time between the defenders of the then customary interpretations of Scripture, and the assertors of the Copernican system of the universe, when the innovators were upbraided with maintaining opinions contrary to Scripture, they replied that Scripture was not intended to teach men astronomy, and that it expressed the acts of divine power in images which were suited to the ideas of unscientific men. To speak of the rising and setting and travelling of the sun, of the fixity and of the foundations of the earth, was to use the only language which would have made the Sacred Narrative intelligible. To extract from these and the like expressions doctrines of science, was, they declared, in the highest degree unjustifiable; and such a course could lead, they held, to no result but a weakening of the authority of Scripture in proportion as its credit was identified with that of these modes of applying it. And this judgment has since been generally assented to by those who most reverence and value the study of the designs of Providence as well as that of the works of nature.

7. *Science tells us nothing concerning Creation.*—Other apparent difficulties arise from the accounts given in the Scripture of the first origin of the world

in which we live: for example, Light is represented as created before the Sun. With regard to difficulties of this kind, it appears that we may derive some instruction from the result to which we were led in the last chapter;—namely, that in the sciences which trace the progress of natural occurrences, we can in no case go back to an origin, but in every instance appear to find ourselves separated from it by a state of things, and an order of events, of a kind altogether different from those which come under our experience. The thread of induction respecting the natural course of the world snaps in our fingers, when we try to ascertain where its beginning is. Since, then, science can teach us nothing positive respecting the beginning of things, she can neither contradict nor confirm what is taught by Scripture on that subject; and thus, as it is unworthy timidity in the lover of Scripture to fear contradiction, so is it ungrounded presumption to look for confirmation, in such cases. The providential history of the world has its own beginning, and its own evidence; and we can only render the system insecure, by making it lean on our material sciences. If any one were to suggest that the nebular hypothesis countenances the Scripture history of the formation of this system, by showing how the luminous matter of the sun might exist previous to the sun itself, we should act wisely in rejecting such an attempt to weave together these two heterogeneous threads;—the one a part of a providential scheme, the other a fragment of a physical speculation.

We shall best learn those lessons of the true philosophy of science which it is our object to collect, by attending to portions of science which have gone through such crises as we are now considering; nor is it requisite, for this purpose, to bring forwards any subjects which are still under discussion. It may, however, be mentioned that such maxims as we are now endeavouring to establish, and the one before us in particular, bear with a peculiar force upon those Palætiological Sciences of which we have been treating in the present Book.

8. *Scientific views, when familiar, do not disturb the authority of Scripture.*—There is another reflection which may serve to console and encourage us in the painful struggles which thus take place, between those who maintain interpretations of Scripture already prevalent and those who contend for such new ones as the new discoveries of science require. It is this;—that though the new opinion is resisted by one party as something destructive of the credit of Scripture and the reverence which is its due, yet, in fact, when the new interpretation has been generally established and incorporated with men's current thoughts, it ceases to disturb their views of the authority of the Scripture or of the truth of its teaching. When the language of Scripture, invested with its new meaning, has become familiar to men, it is found that the ideas which it calls up are quite as reconcileable as the former ones were, with the most entire acceptance of the providential dispensation. And when this has been found to be the case, all cultivated persons look back with surprise at the mistake of those who thought that the essence of the revelation was involved in their own arbitrary version of some collateral circumstance in the revealed narrative. At the present day, we can hardly conceive how reasonable men could ever have imagined that religious reflections on the stability of the earth, and the beauty and use of the luminaries which revolve round it, would be interfered with by an acknowledgment that this rest and motion are apparent only[3]. And thus the authority of revelation is not shaken by any changes introduced by the progress of science in the mode of interpreting expressions which describe physical objects and occurrences; provided the new interpretation is admitted at a proper season, and in a proper spirit; so as to soften, as much as possible, both the public controversies and the private scruples which almost inevitably accompany such an alteration.

9. *When should old Interpretations be given up?*—But the question then occurs, What is the proper

[3] I have here borrowed a sentence or two from my own *History*.

season for a religious and enlightened commentator to make such a change in the current interpretation of sacred Scripture? At what period ought the established exposition of a passage to be given up, and a new mode of understanding the passage, such as is, or seems to be, required by new discoveries respecting the laws of nature, accepted in its place? It is plain, that to introduce such an alteration lightly and hastily would be a procedure fraught with inconvenience; for if the change were made in such a manner, it might be afterwards discovered that it had been adopted without sufficient reason, and that it was necessary to reinstate the old exposition. And the minds of the readers of Scripture, always to a certain extent and for a time disturbed by the subversion of their long-established notions, would be distressed without any need, and might be seriously unsettled. While, on the other hand, a too protracted and obstinate resistance to the innovation, on the part of the scriptural expositors, would tend to identify, at least in the minds of many, the authority of the Scripture with the truth of the exposition; and therefore would bring discredit upon the revealed word, when the established interpretation was finally proved to be untenable.

A rule on this subject, propounded by some of the most enlightened dignitaries of the Roman Catholic church, on the occasion of the great Copernican controversy begun by Galileo, seems well worthy of our attention. The following was the opinion given by Cardinal Bellarmine at the time:—'When a *demonstration* shall be found to establish the earth's motion, it will be proper to interpret the sacred Scriptures otherwise than they have hitherto been interpreted in those passages where mention is made of the stability of the earth and movement of the heavens.' This appears to be a judicious and reasonable maxim for such cases in general. So long as the supposed scientific discovery is doubtful, the exposition of the meaning of Scripture given by commentators of established credit is not wantonly to be disturbed: but when a scientific theory, irreconcileable with this ancient in-

terpretation, is clearly proved, we must give up the interpretation, and seek some new mode of understanding the passage in question, by means of which it may be consistent with what we know; for if it be not, our conception of the things so described is no longer consistent with itself.

It may be said that this rule is indefinite, for who shall decide when a new theory is completely demonstrated, and the old interpretation become untenable? But to this we may reply, that if the rule be assented to, its application will not be very difficult. For when men have admitted as a general rule, that the current interpretations of scriptural expressions respecting natural objects and events may possibly require, and in some cases certainly will require, to be abandoned, and new ones admitted, they will hardly allow themselves to contend for such interpretations as if they were essential parts of revelation; and will look upon the change of exposition, whether it come sooner or later, without alarm or anger. And when men lend themselves to the progress of truth in this spirit, it is not of any material importance at what period a new and satisfactory interpretation of the scriptural difficulty is found; since a scientific exactness in our apprehension of the meaning of such passages as are now referred to is very far from being essential to our full acceptance of revelation.

10. *In what Spirit should the Change be accepted?* —Still these revolutions in scriptural interpretation must always have in them something which distresses and disturbs religious communities. And such uneasy feelings will take a different shape, according as the community acknowledges or rejects a paramount interpretative authority in its religious leaders. In the case in which the interpretation of the Church is binding upon all its members, the more placid minds rest in peace upon the ancient exposition, till the spiritual authorities announce that the time for the adoption of a new view has arrived; but in these circumstances, the more stirring and inquisitive minds, which cannot refrain from the pursuit of new truths

and exact conceptions, are led to opinions which, being contrary to those of the Church, are held to be sinful. On the other hand, if the religious constitution of the community allow and encourage each man to study and interpret for himself the Sacred Writings, we are met by evils of another kind. In this case, although, by the unforced influence of admired commentators, there may prevail a general agreement in the usual interpretation of difficult passages, yet as each reader of the Scripture looks upon the sense which he has adopted as being his own interpretation, he maintains it, not with the tranquil acquiescence of one who has deposited his judgment in the hands of his Church, but with the keenness and strenuousness of self-love. In such a state of things, though no judicial severities can be employed against the innovators, there may arise more angry controversies than in the other case.

It is impossible to overlook the lesson which here offers itself, that it is in the highest degree unwise in the friends of religion, whether individuals or communities, unnecessarily to embark their credit in expositions of Scripture on matters which appertain to natural Science. By delivering physical doctrines as the teaching of revelation, religion may lose much, but cannot gain anything. This maxim of practical wisdom has often been urged by Christian writers. Thus St. Augustine says[4]: 'In obscure matters and things far removed from our senses, if we read anything, even in the divine Scripture, which may produce diverse opinions without damaging the faith which we cherish, let us not rush headlong by positive assertion to either the one opinion or the other; lest, when a more thorough discussion has shown the opinion which we had adopted to be false, our faith may fall with it: and we should be found contending, not for the doctrine of the sacred Scriptures, but for our own; endeavouring to make our doctrine to be that of the Scriptures, instead of taking the doctrine of the Scriptures to be ours.' And in nearly the same spirit, at the

[4] Lib. i. *de Genesi*, cap. xviii.

time of the Copernican controversy, it was thought proper to append to the work of Copernicus a postil, to say that the work was written to account for the phenomena, and that people must not run on blindly and condemn either of the opposite opinions. Even when the Inquisition, in 1616, thought itself compelled to pronounce a decision upon this subject, the verdict was delivered in very moderate language;—that 'the doctrine of the earth's motion appeared to be contrary to Scripture:' and yet, moderate as this expression is, it has been blamed by judicious members of the Roman church as deciding a point such as religious authorities ought not to pretend to decide; and has brought upon that church no ordinary weight of general condemnation. Kepler pointed out, in his lively manner, the imprudence of employing the force of religious authorities on such subjects: *Acies dolabræ in ferrum illisa, postea nec in lignum valet amplius. Capiat hoc cujus interest.* 'If you *will* try to chop iron, the axe becomes unable to cut even wood. I warn those whom it concerns.'

11. *In what Spirit should the Change be urged?*—But while we thus endeavour to show in what manner the interpreters of Scripture may most safely and most properly accept the discoveries of science, we must not forget that there may be errours committed on the other side also; and that men of science, in bringing forward views which may for a time disturb the minds of lovers of Scripture, should consider themselves as bound by strict rules of candour, moderation, and prudence. Intentionally to make their supposed discoveries a means of discrediting, contradicting, or slighting the sacred Scriptures, or the authority of religion, is in them unpardonable. As men who make the science of Truth the business of their lives, and are persuaded of her genuine superiority, and certain of her ultimate triumph, they are peculiarly bound to urge her claims in a calm and temperate spirit; not forgetting that there are other kinds of truth besides that which they peculiarly study. They may properly reject authority in matters of science; but they are to leave

it its proper office in matters of religion. I may here again quote Kepler's expressions: 'In Theology we balance authorities, in Philosophy we weigh reasons. A holy man was Lactantius who denied that the earth was round; a holy man was Augustine, who granted the rotundity, but denied the antipodes; a holy thing to me is the Inquisition, which allows the smallness of the earth, but denies its motion; but more holy to me is Truth; and hence I prove, from philosophy, that the earth is round, and inhabited on every side, of small size, and in motion among the stars,—and this I do with no disrespect to the Doctors.' I the more willingly quote such a passage from Kepler, because the entire ingenuousness and sincere piety of his character does not allow us to suspect him in anything of hypocrisy or latent irony. That similar professions of respect may be made ironically, we have a noted example in the celebrated Introduction to *Galileo's Dialogue on the Copernican System;* probably the part which was most offensive to the authorities. 'Some years ago,' he begins, 'a wholesome edict was promulgated at Rome, which, in order to check the perilous scandals of the present age, imposed silence upon the Pythagorean opinion of the mobility of the earth. There were not wanting,' he proceeds, 'persons who rashly asserted that this decree was the result, not of a judicious inquiry, but of passion ill-informed; and complaints were heard that councillors, utterly unacquainted with astronomical observation, ought not to be allowed, with their sudden prohibitions, to clip the wings of speculative intellects. *At the hearing of rash lamentations like these, my zeal could not keep silence.*' And he then goes on to say, that he wishes, in his *Dialogue*, to show that the subject had been fully examined at Rome. Here the irony is quite transparent, and the sarcasm glaringly obvious. I think we may venture to say that this is not the temper in which scientific questions should be treated; although by some, perhaps, the prohibition of public discussion may be considered as justifying any evasion which is likely to pass unpunished.

12. *Duty of Mutual Forbearance.*—We may add, as a further reason for mutual forbearance in such cases, that the true interests of both parties are the same. The man of science is concerned, no less than any other person, in the truth and import of the divine dispensation; the religious man, no less than the man of science, is, by the nature of his intellect, incapable of believing two contradictory declarations. Hence they have both alike a need for understanding the Scripture in some way in which it shall be consistent with their understanding of nature. It is for their common advantage to conciliate, as Kepler says, the finger and the tongue of God, his works and his word. And they may find abundant reason to bear with each other, even if they should adopt for this purpose different interpretations, each finding one satisfactory to himself; or if any one should decline employing his thoughts on such subjects at all. I have elsewhere[5] quoted a passage from Kepler[6] which appears to me written in a most suitable spirit: 'I beseech my reader that, not unmindful of the divine goodness bestowed upon man, he do with me praise and celebrate the wisdom of the Creator, which I open to him from a more inward explication of the form of the world, from a searching of causes, from a detection of the errours of vision; and that thus not only in the firmness and stability of the earth may we perceive with gratitude the preservation of all living things in nature as the gift of God: but also that in its motion, so recondite, so admirable, we may acknowledge the wisdom of the Creator. But whoever is too dull to receive this science, or too weak to believe the Copernican system without harm to his piety, him, I say, I advise that, leaving the school of astronomy, and condemning, if so he please, any doctrines of the philosophers, he follow his own path, and desist from this wandering through the universe; and that, lifting up his natural eyes, with which alone he can see,

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[5] *Bridgewater Tr.* p. 314.

[6] *Com. Stell. Mart.* Introd.

he pour himself out from his own heart in worship of God the Creator, being certain that he gives no less worship to God than the astronomer, to whom God has given to see more clearly with his inward eyes, and who, from what he has himself discovered, both can and will glorify God.'

13. *Case of Galileo.*—I may perhaps venture here to make a remark or two upon this subject with reference to a charge brought against a certain portion of the *History of the Inductive Sciences.* Complaint has been made[7] that the character of the Roman church, as shown in its behaviour towards Galileo, is misrepresented in the account given of it in the History of Astronomy. It is asserted that Galileo provoked the condemnation he incurred; first, by pertinaciously demanding the assent of the ecclesiastical authorities to his opinion of the consistency of the Copernican doctrine with Scripture; and afterwards by contumaciously, and, as we have seen, contumeliously violating the silence which the Church had enjoined upon him. It is further declared that the statement which represents it as the habit of the Roman church to dogmatize on points of natural science is unfounded; as well as the opinion that in consequence of this habit, new scientific truths were promulgated less boldly in Italy than in other countries. I shall reply very briefly on these subjects; for the decision of them is by no means requisite in order to establish the doctrines to which I have been led in the present chapter, nor, I hope, to satisfy my reader that my views have been collected from an impartial consideration of scientific history.

With regard to Galileo, I do not think it can be denied that he obtruded his opinions upon the ecclesiastical authorities in an unnecessary and imprudent manner. He was of an ardent character, strongly convinced himself, and urged on still more by the conviction which he produced among his disciples, and

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[7] *Dublin Review*, No. ix. July, 1838, p. 72.

thus he became impatient for the triumph of truth. This judgment of him has recently been delivered by various independent authorities, and has undoubtedly considerable foundation[8]. As to the question whether authority in matters of natural science were habitually claimed by the authorities of the Church of Rome, I have to allow that I cannot produce instances which establish such a habit. We, who have been accustomed to have daily before our eyes the Monition which the Romish editors of Newton thought it necessary to prefix—*Cæterum latis a summo Pontifice contra telluris motum Decretis, nos obsequi profitemur*—were not likely to conjecture that this was a solitary instance of the interposition of the Papal authority on such subjects. But although it would be easy to find declarations of heresy delivered by Romish Universities, and writers of great authority, against tenets belonging to the natural sciences, I am not aware that any other case can be adduced in which the Church or the Pope can be shown to have pronounced such a sentence. I am well contented to acknowledge this; for I should be far more gratified by finding myself compelled to hold up the seventeenth century as a model for the nineteenth in this respect, than by having to sow enmity between the admirers of the past and the present through any disparaging contrast[9].

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[8] Besides the *Dublin Review*, I may quote the *Edinburgh Review*, which I suppose will not be thought likely to have a bias in favour of the exercise of ecclesiastical authority in matters of science; though certainly there is a puerility in the critic's phraseology which does not add to the weight of his judgment. 'Galileo contrived to surround the truth with every variety of obstruction. The tide of knowledge, which had hitherto advanced in peace, he crested with angry breakers, and he involved in its surf both his friends and his foes.' —*Ed. Rev.* No. cxxiii. p. 126.

[9] I may add that the most candid of the adherents of the Church of Rome condemn the assumption of authority in matters of science, made, in this one instance at least, by the ecclesiastical tribunals. The author of the *Ages of Faith* (book viii. p. 248), says, 'A Congregation, it is to be lamented, declared the new system to be opposed to Scripture, and therefore heretical.'

With respect to the attempt made in my History to characterize the intellectual habits of Italy as produced by her religious condition,—certainly it would ill become any student of the history of science to speak slightingly of that country, always the mother of sciences, always ready to catch the dawn and hail the rising of any new light of knowledge. But I think our admiration of this activity and acuteness of mind is by no means inconsistent with the opinion, that new truths were promulgated more boldly beyond the Alps, and that the subtilty of the Italian intellect loved to insinuate what the rough German bluntly asserted. Of the decent duplicity with which forbidden opinions were handled, the reviewer himself gives us instances, when he boasts of the liberality with which Copernican professors were placed in important stations by the ecclesiastical authorities, soon after the doctrine of the motion of the earth had been declared by the same authorities to be contrary to Scripture. And in the same spirit is the process of demanding from Galileo a public and official recantation of opinions which he had repeatedly been told by his ecclesiastical superiors he might hold as much as he pleased. I think it is easy to believe that among persons so little careful to reconcile public profession with private conviction, official decorum was all that was demanded. When Galileo had made his renunciation of the earth's motion on his knees, he rose and said, as we are told, *E pur si muove* —'and yet it *does* move.' This is sometimes represented as the heroic soliloquy of a mind cherishing its conviction of the truth, in spite of persecution; I think we may more naturally conceive it uttered as a playful epigram in the ear of a cardinal's secretary, with a full knowledge that it would be immediately repeated to his master[10].

Besides the Ideas involved in the material sciences,

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[10] I have somewhat further discussed the case of Galileo in the later editions of the *History*, book v. chap. iii. sect. 4.

of which we have already examined the principal ones, there is one Idea or Conception which our Sciences do not indeed include, but to which they not obscurely point; and the importance of this Idea will make it proper to speak of it, though this must be done very briefly.

CHAPTER V.

OF THE CONCEPTION OF A FIRST CAUSE.

1. AT the end of the last chapter but one, we were led to this result,—that we cannot, in any of the Palætiological Sciences, ascend to a beginning which is of the same nature as the existing cause of events, and which depends upon causes that are still in operation. Philosophers never have demonstrated, and probably never will be able to demonstrate, what was the original condition of the solar system, of the earth, of the vegetable and animal worlds, of languages, of arts. On all these subjects the course of investigation, followed backwards as far as our materials allow us to pursue it, ends at last in an impenetrable gloom. We strain our eyes in vain when we try, by our natural faculties, to discern an origin.

2. Yet speculative men have been constantly employed in attempts to arrive at that which thus seems to be placed out of their reach. The Origin of Languages, the Origin of the present Distribution of Plants and Animals, the Origin of the Earth, have been common subjects of diligent and persevering inquiry. Indeed inquiries respecting such subjects have been, at least till lately, the usual form which Palætiological researches have assumed. *Cosmogony*, the Origin of the World, of which, in such speculations, the earth was considered as a principal part, has been a favourite study both of ancient and of modern times: and most of the attempts at Geology previous to the present period have been *Cosmogonies* or *Geogonies*, rather than that more genuine science which we have endeavoured to delineate. Again: Glossology, though now an extensive body of solid knowledge, was

mainly brought into being by inquiries concerning the Original Language spoken by men; and the nature of the first separation and diffusion of languages, the first peopling of the earth by man and by animals, were long sought after with ardent curiosity, although of course with reference to the authority of the Scriptures, as well as the evidence of natural phenomena. Indeed the interest of such inquiries even yet is far from being extinguished. The disposition to explore the past in the hope of finding, by the light of natural reasoning as well as by the aid of revelation, the origin of the present course of things, appears to be unconquerable. 'What was the beginning?' is a question which the human race cannot desist from perpetually asking. And no failure in obtaining a satisfactory answer can prevent inquisitive spirits from again and again repeating the inquiry, although the blank abyss into which it is uttered does not even return an echo.

3. What, then, is the reason of an attempt so pertinacious yet so fruitless? By what motive are we impelled thus constantly to seek what we can never find? Why are the errour of our conjectures, the futility of our reasonings, the precariousness of our interpretations, over and over again proved to us in vain? Why is it impossible for us to acquiesce in our ignorance and to relinquish the inquiry? Why cannot we content ourselves with examining those links of the chain of causes which are nearest to us,—those in which the connexion is intelligible and clear; instead of fixing our attention upon those remote portions where we can no longer estimate its coherence? In short, why did not men from the first take for the subject of their speculations the Course of Nature rather than the Origin of Things?

To this we reply, that in doing what they have thus done, in seeking what they have sought, men are impelled by an intellectual necessity. They cannot conceive a Series of connected occurrences without a Commencement; they cannot help supposing a cause for the Whole, as well as a cause for each part; they cannot be satisfied with a succession of causes without

assuming a First Cause. Such an assumption is necessarily impressed upon our minds by our contemplation of a series of causes and effects; that *there must be a First Cause*, is accepted by all intelligent reasoners as an Axiom: and like other Axioms, its truth is necessarily implied in the Idea which it involves.

4. The evidence of this axiom may be illustrated in several ways. In the first place, the axiom is assumed in the argument usually offered to prove the existence of the Deity. Since, it is said, the world now exists, and since nothing cannot produce something, something must have existed from eternity. This Something is the First Cause: it is God.

Now what I have to remark here is this:—the conclusiveness of this argument, as a proof of the existence of one independent, immutable Deity, depends entirely upon the assumption of the axiom above stated. The World, a serious of causes and effects, exists: therefore there must be, not only this series of causes and effects, but also a First Cause. It will be easily seen, that without the axiom, that in every series of causes and effects there must be a First Cause, the reasoning is altogether inconclusive.

5. Or to put the matter otherwise: The argument for the existence of the Deity was stated thus: Something exists, therefore something must have existed from eternity. 'Granted,' the opponent might say; 'but this *something* which has existed from eternity, why may it not be this very series of causes and effects which is now going on, and which appears to contain in itself no indication of beginning or end?' And thus, without the assumption of the necessity of a First Cause, the force of the argument may be resisted.

6. But, it may be asked, how do those who have written to prove the existence of the Deity reply to such an objection as the one just stated? It is natural to suppose that, on a subject so interesting and so long discussed, all the obvious arguments with their replies, have been fully brought into view. What is the result in this case?

The principal modes of replying to the above objection, that the series of causes and effects which now exists, may have existed from eternity, appear to be these.

In the first place, our minds cannot be satisfied with a series of successive, dependent, causes and effects, without something first and independent. We pass from effect to cause, and from that to a higher cause, in search of something on which the mind can rest; but if we can do nothing but repeat this process, there is no use in it. We move our limbs, but make no advance. Our question is not answered, but evaded. The mind cannot acquiesce in the destiny thus presented to it, of being referred from event to event, from object to object, along an interminable vista of causation and time. Now this mode of stating the reply,—to say that the mind *cannot thus be satisfied*, appears to be equivalent to saying that the mind is conscious of a Principle, in virtue of which such a view as this must be rejected;—the mind takes refuge in the assumption of a First Cause, from an employment inconsistent with its own nature.

7. Or again, we may avoid the objection, by putting the argument for the existence of a Deity in this form: The series of causes and effects which we call the *world*, or the *course of nature*, may be considered as a *whole*, and this whole must have a cause of its existence. The whole collection of objects and events may be comprehended as a single effect, and of this effect there must be a cause. This Cause of the Universe must be superior to, and independent of the special events, which, happening in time, make up the universe of which He is the cause. He must exist and exercise causation, before these events can begin: He must be the First Cause.

Although the argument is here somewhat modified in form, the substance is the same as before. For the assumption that we may consider the whole series of causes and effects as a *single effect*, is equivalent to the assumption that besides partial causes we must have a First Cause. And thus the Idea of a First Cause, and

the axiom which asserts its necessity, are recognized in the usual argumentation on this subject.

8. This Idea of a First Cause, and the principle involved in the Idea, have been the subject of discussion in another manner. As we have already said, we assume as an axiom that a First Cause must exist; and we assert that God, the First Cause, exists eternal and immutable, by the necessity which the axiom implies. Hence God is said to exist necessarily;—to be a necessarily existing being. And when this *necessary existence* of God had been spoken of, it soon began to be contemplated as a sufficient reason, and as an absolute demonstration of His existence; without any need of referring to the world as an effect, in order to arrive at God as the cause. And thus men conceived that they had obtained a proof of the existence of the Deity, *à priori*, from Ideas, as well as *à posteriori*, from Effects.

9. Thus, Thomas Aquinas employs this reasoning to prove the *eternity* of God[1]: 'Oportet ponere aliquod primum necessarium quod est per se ipsum necessarium; et hoc est Deus, cum sit prima causa ut dictum est: igitur Deus æternus est, cum omne necessarium per se sit æternum.' It is true that the schoolmen never professed to be able to prove the *existence* of the Deity *à priori:* but they made use of this conception of necessary existence in a manner which approached very near to such an attempt. Thus Suarez[2] discusses the question, 'Utrum aliquo modo possit *à priori* demonstrari Deum esse.' And resolves the question in this manner: 'Ad hunc ergo modum dicendum est: Demonstrato *à posteriori* Deum esse ens necessarium et a se, ex hoc attributo posse *à priori* demonstrari præter illud non posse esse aliud ens necessarium et a se, et consequenter demonstrari Deum esse.'

But in modern times attempts were made by Descartes and Samuel Clarke, to prove the Divine exist-

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1 Aquin. *Cont. Gentil.* lib. i. c. xiv. p. 21.

2 *Metaphys.* tom. ii. disp. xxix. sect. 3, p. 28.

ence at once *à priori*, from the conception of necessary existence; which, it was argued, could not subsist without actual existence. This argumentation was acutely and severely criticised by Dr. Waterland.

10. Without dwelling upon a subject, the discussion of which does not enter into the design of the present work, I may remark that the question whether an *à priori* proof of the existence of a First Cause be possible, is a question concerning the nature of our Ideas, and the evidence of the axioms which they involve, of the same kind as many questions which we have already had to discuss. Is our Conception or Idea of a First Cause gathered from the effects we see around us? It is plain that we must answer, here as in other cases, that the Idea is not extracted from the phenomena, but assumed in order that the phenomena may become intelligible to the mind;—that the Idea is a necessary one, inasmuch as it does not depend upon observation for its evidence; but that it depends upon observation for its development, since without some observation, we cannot conceive the mind to be cognizant of the relation of causation at all. In this respect, however, the Idea of a First Cause is no less necessary than the ideas of Space, or Time, or Cause in general. And whether we call the reasoning derived from such a necessity an argument *à priori* or *à posteriori*, in either case it possesses the genuine character of demonstration, being founded upon axioms which command universal assent.

11. I have, however, spoken of our *Conception* rather than of our *Idea* of a First Cause; for the notion of a First Cause appears to be rather a modification of the Fundamental Idea of Cause, which was formerly discussed, than a separate and peculiar Idea. And the Axiom, *that there must be a First Cause*, is recognised by most persons as an application of the general Axiom of Causation, *that every effect must have a Cause;* this latter Axiom being applied to the World, considered in its totality, as a single Effect. This distinction, however, between an Idea and a Conception, is of no material consequence to our argument; provided we

allow the maxim, that there must be a First Cause, to be necessarily and evidently true; whether it be thought better to speak of it as an independent Axiom, or to consider it as derived from the general Axiom of Causation.

12. Thus we necessarily infer a First Cause, although the Palætiological Sciences only point *towards* it, and do not lead us *to* it. But I must observe further; that in each of the series of events which form the subject of Palætiological research, the First Cause is the *same*. Without here resting upon reasoning founded upon our Conception of a First Cause, I may remark that this identity is proved by the close connexion of all the branches of natural science, and the way in which the causes and the events of each are interwoven with those which belong to the others. We must needs believe that the First Cause which produced the earth and its atmosphere is also the Cause of the plants which clothe its surface; that the First Cause of the vegetable and of the animal world are the same; that the First Cause which produced light produced also eyes; that the First Cause which produced air and organs of articulation produced also language and the faculties by which language is rendered possible: and if *those* faculties, then also all man's other faculties;—the powers by which, as we have said, he discerns right and wrong, and recognises a providential as well as a natural course of things. Nor can we think otherwise than that the Being who gave these faculties, bestowed them for some purpose;—bestowed them for that purpose which alone is compatible with their nature:—the purpose, namely, of guiding and elevating man in his present career, and of preparing him for another state of being to which they irresistibly direct his hopes. And thus, although, as we have said, no one of the Palætiological Sciences can be traced continuously to an Origin, yet they not only each point to an Origin, but all to the same Origin. Their lines are broken indeed, as they run backwards into the early periods of the world, but yet they all appear to converge to the same invisible point. And

this point, thus indicated by the natural course of things, can be no other than that which is disclosed to us as the starting-point of the providential course of the world; for we are persuaded by such reasons as have just been hinted, that the Creator of the natural world can be no other than the Author and Governor and Judge of the moral and spiritual world.

13. Thus we are led, by our material Sciences, and especially by the Palætiological class of them, to the borders of a higher region, and to a point of view from which we have a prospect of other provinces of knowledge;—to contemplations in which other faculties of man are concerned besides his intellectual, other interests involved besides those of speculation. On these it does not belong to our present plan to dwell: but even such a brief glance as we have taken of the connexion of material with moral speculations may not be useless, since it may serve to show that the principles of truth which we are now laboriously collecting among the results of the physical sciences, may possibly find some application in those parts of knowledge towards which men most naturally look with deeper interest and more serious reverence.

We have been employed hitherto in examining the materials of knowledge, Facts and Ideas;—Facts in our former History, and Ideas in the present History. We have dwelt at length on this latter element; inasmuch as the consideration of it is, on various accounts, and especially at the present time, by far the most important, having hitherto been least distinctly attended to as a special element of scientific knowledge.

There still remains an important task, with a view to which we have undertaken this survey of the past course of human thought and discovery:—namely, the task of determining the processes by which these materials may actually be made to constitute knowledge.

We have surveyed the stones which lie before us, partly built and partly ready for building: we have found them exactly squared, and often curiously covered with significant imagery and important inscriptions. We have now to discover how they may best be fitted into their places, and cemented together, so that rising stage above stage, they may grow at last into that fair and lofty temple of Truth, for which we cannot doubt that they were intended by the Great Architect.

This task, the description of the processes by which Scientific Truth is discovered and established, we shall, as has already been said, entitle, in reference to previous attempts of the same kind, *Novum Organum Renovatum.*

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